WAR AND BEHAVIOR
Evidence from Angolan Civil War Veterans

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Abstract

Military service in conflict zones strongly marks the lives of millions of people across the world. The objective of this thesis is to contribute to understanding how and why young men are exposed to practices and policies by armed groups, and how these experiences affect their behavior in the long run. The thesis presents three self-contained essays that all address this objective theoretically and empirically. The empirical analysis relies on primary survey data from 760 Angolan veterans, uses deep insights into the local context and exploits the Angolan Civil War as a natural experiment to identify causal mechanisms.

Chapter 2 argues that a non-state armed actor may – like a state actor – have the ability to tax a local population and will choose conscription as its primary recruitment policy. Chapter 3 exposes that former soldiers who were more involved in local governance during the war are significantly more likely to participate in collective public good production more than a decade after the end of the war. Gaining experience with social cooperation increases contributions to organizing public goods, while a shift in political preferences fosters participation in their delivery. Chapter 4 shows that exposure to sexual violence by armed groups significantly increases an individual's long-term propensity to commit violence against an intimate partner. This effect is underpinned by a reduction of psychological barriers to violence against women.

The thesis contributes to several literatures on the long-run individual-level origins of human behavior and on the conduct and consequences of armed conflict. The findings challenge conventional models of rebel recruitment, dominant theories of domestic violence, and existing knowledge of how war affects local institutions. The findings can also help policymakers to tackle and leverage long-run impacts of conflict on behavior, in order to devise new policies for more cooperative and less violent societies.

Acknowledgements

Just a few years ago, I was a Diplom student in physics, enthusiastically researching how electrons interact. It was fantastic – but I became curious about humans. A lot of coursework in economics and prolonged fieldwork in Angola would follow. It was excruciating at times, but it was also the most exciting and rewarding journey I could imagine. Its success is the result of the encouragement, support and advice I received from many individuals, groups and institutions.

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Chapter 1

Introduction

1.1 Objective

Military service in armed conflict strongly marks the lives of millions of young people across the world. The objective of this thesis is to contribute to understanding what young men experience as soldiers in armed conflict and how the military service affects their behavior in the long run.

The thesis will present three self-contained essays that all address this objective theoretically and empirically. The first essay (Chapter 2) will develop and test a theory of rebel recruitment that is primarily based on tax compliance rather than individual motivation or the use of physical force by the armed group. The second and third essay will examine causal long-term effects of individual exposure to benign and malign forms of conflict policies and practices adopted by armed groups: Chapter 3 will study the long-term impact of exposure to public good delivery to civilians with the armed group on post-service participation in collective public good production. Chapter 4 will consider the long-term impact of exposure to sexual violence against civilian women on perpetrating post-service intimate partner violence.

The empirical analysis detailed is based on self-collected survey data from 760 government and rebel veterans of the Angolan Civil War, twelve years after the end of the war. The empirical evidence will thus be derived from military service with a civil war actor, but all arguments will be made for military service with an armed actor engaged in armed conflict more generally.

1.2 Motivation

The questions of military service - who participates, why, how, and at what long-run costs and benefits - are important for the conceptual understanding of armed conflict, and its relationship with post-conflict social cohesion, political stability, and economic development.

It is well known that armed actors often penetrate the lives of their soldiers and those of millions of civilians in particularly violent ways, including fatal violence and wartime rape. Yet many actors have now also been shown to interact with civilians in "positive" ways, some of which are considered an expression of rudimentary forms of state building. These interactions include establishing stable local forms of a legitimate monopoly of violence, public good provision and taxation. Notably, any such group policy or practice – positive or negative – is eventually executed or practiced by its soldiers, but systematic micro-analyses of conflict and military service experiences beyond veteran-non veteran comparisons and the exposure to battle violence are very rare.

The realization of many conflict policies – especially positive ones – hinges on a certain degree of 'institutional capacity', defined as a more general equivalent to 'state capacity' – an actors' ability to choose and implement such policies. Non-state conflict actors, however, have traditionally been assumed to have 'low' institutional capacity. As an example, the dominant view in the conflict literature conceptualizes insurgency - the most dominant type of armed conflict in the post-World War II era - as "a technology of military conflict characterized by small, lightly armed bands practicing guerrilla warfare from rural base areas" (Fearon and Laitin, 2003, 75).

After conflicts end, former members of armed groups and forces are – due to their conflict experiences and the legacies of these – often feared to be a source of instability, and potential micro-engines of conflict and poverty cycles at the individual and more aggregate levels. Yet causal and disaggregated analyses of the consequences of armed conflict military service for post-conflict individuals, families and societies are largely absent.

The motivation for the research presented in this thesis is thus to improve the understanding of three important themes related to military service in armed conflict: what it really means, how it is shaped by armed actors' capacity and choices, and – as the primary focus of the thesis – what its long-term legacies are.

1.3 Research Design

1.3.1 Methodological concept

This project chose a micro-level approach to study these themes. The main unit of analysis will be the individual, whose experiences during military service are (partially) shaped by variation in policies and practices at the armed group-level. The themes will be studied based on theory from economics and related disciplines, self-collected survey data, and statistical data analysis.

The study will draw on economic, political, psychological, and sociological research to derive hypotheses for two sets of mechanisms shaping individual-level outcomes. First, mechanisms *creating* variation in military service experiences (set 1), and second, mechanisms *linking* variation in military service experiences to long-term behavior (set 2).

The empirical data employed are original survey data collected by the author in Huambo province, Angola, in 2013 and 2014. The survey was part of the Study of Angolan Ex-Combatant (POEMA) project, and was preceded by one year of ethnographic fieldwork by a doctoral student in anthropology (Spall, 2015).

The statistical data analysis will be partly guided by the hypothesized theoretical mechanisms in set 1 to produce parameter estimates with a causal interpretation. The project-based anthropological accounts were used to assess the validity of the theoretical hypotheses, determine their relevance in the local context, refine the survey questionnaire design, interpret quantitative results and explore underlying mechanisms of set 2.

Important motivations for and implications of some of these choices will now be briefly discussed in a bit more detail below.

1.3.2 Micro-level concept

The quantitative and economic analysis of armed conflicts and their effects is a relatively recent field, which has been dominated by comparative analyses of the differences in aggregate outcomes across regions and time. While important, such approaches are limited in explaining the role of the micro-level processes that underlie armed conflict and their relationship with social, economic, and political outcomes at the micro- and aggregate levels.

The thesis recognizes that at the heart of military institutions and organizing large-scale violence is collective action by individuals. From this perspective, understanding participation and service in armed groups hence requires a joint analysis of (the incentives and constraints at) the individual and collective (i.e.

group) levels.

In a similar sense, the thesis emphasizes that at the heart of the conduct of military operations is a principal-agent setup, with a group commander as the principal, and a soldier as the agent. Abstracting from questions of 'the commander's dilemma', commanders can either order, prohibit or tolerate actions by the soldiers (e.g. Wood, 2015). When soldiers hence implement an order or are explicitly or implicitly prohibited from certain actions they are - as termed here - 'exposed to a group *policy*'. Situations where the group tolerates a certain category of actions is then best described a group *practice*. From this perspective, understanding the origins and consequences of individual service experiences hence requires an examination of individual exposure to group policies and practices.

1.3.3 Case Selection

Angola's recent history offers a suitable setting for this study. Between 1975 and 2002, Angola experienced a very long, large-scale and intense military conflict. Yet, it was fought by the same two factions throughout, both - as history has shown - with means to orchestrate mass mobilization. The rival actors were the *Movimento Popular de Libertação de Angola* (MPLA) government and the *União Nacional para a Independência Total de Angola* (UNITA) rebels. This combination suggests the population of former soldiers is likely to be large and contain substantial variation in individual military experiences.

On the other hand, using data from 2013/2014 allows to study long-run consequences of service experiences on behavior more than a decade after the end of the war. While Angola has now experienced more than a decade of enormous economic growth, this has been based almost exclusively on crude oil revenues. State institutions and their influence remain weak, stifling effective political and economic development. In practice, this means, that in most regions outside the capital, governance is still a local and collective matter due to the very limited regional penetration by the central state. In comparison to countries of comparable development status, the number of non-governmental organizations (NGO), foreign aid projects and their influence are also very small in Angola.

These characteristics suggest that collective public good production will be an observable and very relevant outcome, and that the estimates of the long-run effects of service experience will credibly not be conflated by post-conflict treatments, which may not be independent from service experiences.

The study is focused on Huambo province ('Huambo' hereafter)[1] - the heart of the Angolan Central Highlands - for five main reasons.

[1] Huambo province is roughly of the same size as Switzerland and has a total resident population of about 2.5 million.

First, this region was at the center of the war. It was in Huambo City, Angola's second largest city, that UNITA proclaimed their own government on the day MPLA declared the independence of Angola, on 11 November 1975. Most parts of the vast Central Highlands were occupied by the two movements at different times and changed hands multiple times, which makes it possible to compare directly the members, practices and dynamics of the rival organizations.

Second, this region was *not* at the center of the previous colonial war against the Portuguese rulers from 1961 to 1974. Most of the anti-colonial activity and guerrilla fighting took place in regions far away from Huambo, and both movements started most of their activities and mobilization strategies in Huambo only after the end of the colonial war. In 1974, the rushed exodus of the Portuguese administration, and Angolans employed in it, following the coup d'état in Portugal, led to a literal collapse of 'the state' in the whole Angolan country. Hence, this offers an opportunity to study conflict policies and practices related to civilians in a relatively 'non-confounded' way, as they started operating in an institutional quasi-vacuum.

Third, the Angola literature reports that in this region both groups frequently and systematically established local territorial monopolies over force and engaged with the governed populations. It portrays both factions as highly capable actors with a state-building narrative and refined strategies to legitimize their mission, win the people's 'hearts and minds', and establish political systems at the local level. Huambo is thus a promising case to study conflict policies and practices related to civilians by actors with high institutional capacity.

Fourth, Huambo is the most densely populated region in Angola but ethnically homogeneous. While the Angola literature argues that ethnicity was never at the root of the conflict, this design allows to rule out confounding individual factors related to ethnicity.

Fifth, building on the previous two points, the Angola literature suggests that the Angolan War created a natural experiment for young men in the Central Highlands. The literature documents that control of local territory shifted frequently and concludes that *"a person's first contact with any political formation was as likely to have been with UNITA as with the MPLA"* (Pearce, 2012, 463). Pearce (2009, 4–5) adds that *"political identity was a matter of necessity rather than of conviction. It is for this reason that I use the word 'adherent' rather than 'supporter' when referring to the people who lived under the control of one or other movement during the war, since 'support' suggests a degree of voluntary affiliation which misrepresents the relationship."*

What did this mean for participation in armed groups? There is abundant evidence that both actors were able to mobilize *en masse*. In state-controlled regions, military service was compulsory by law starting in 1976, and the law specified that military service was obligatory for men between the 1st of January of the year they turned 20 and the 31st of December of the year they turned 45 (Junior, 2015). There are no coherent reports for how UNITA recruited, which will be analyzed in detail to test the

recruitment theory put forth in Chapter 2. Anecdotal evidence, however, suggests that as in MPLA areas practically all able-bodied young man in UNITA-controlled areas joined the army. If true, this suggests the following natural experiment for young men who joined the military: *when* an individual would join the army was strongly determined by when he was born, and second, *which army* he would join was strongly determined by which army controlled his territory when he became an able-bodied young man.

1.3.4 Identification

To mitigate endogeneity bias in the estimated effects of military service, the study ultimately relies on instrumental variable estimation. The adopted instrumental variable strategy combines the natural experiment just described with temporal variation in armed group policies and practices.

Drawing on theory, history and conflict-event data, Chapter 3 will hypothesize that the group-level extent of public good provision to civilians varied over time, Chapter 4 will hypothesize that group-level extent of sexual violence against civilian women varied over time. The basic arguments for these hypotheses are: 1) Group-level public good provision was more likely in times of more territorial gains, as then groups have an incentive to invest (more) in relations with new local populations to secure their support, and 2) group-level sexual violence was more likely in times of more non-political operations against civilians. We exploit the fact that most operations against civilians included (non-fatal) non-sexual violence, such as looting, while mostly *political* operations included fatal violence, such as massacres.

Based on the approach to exposure to group policies and practices presented above the basic idea, then, is as follows: A soldier who served in distinct periods in which *his army* engaged (more) in a certain policy or practice, was, on average, more likely to be (more) exposed to the policy or practice. Combined with the insight that date of entry was strongly predicted by date of birth, this suggests, put simply, that individuals were born into a policy exposure 'premium' or 'penalty', defined by the distinct variation at the group level in the policy.

This defines the identifying variation: The *interaction* between the army the individual joined and his year of birth will be argued to be an informative source of variation in exposure to a certain policy or practice. The identifying statistical assumption is that this source of variation is uncorrelated with unobserved individual confounding factors.

1.3.5 Data Collection and Limitations

As noted above, fieldwork was supported by an anthropological companion study, based on twelve months of fieldwork preceding the survey. Ethnographic fieldwork started in January 2012 and was carried out by John Spall of Sussex University (Spall 2015).

Survey fieldwork started in March 2013 and ended in February 2014 and was done in partnership with the Angolan NGO *Development Workshop* (DW). Despite the generally very modest level of NGO activity in Angola, DW has operated in Angola and Huambo Province for more than 25 years and was instrumental in making this survey possible.[2] With DW's support, the author recruited, screened, trained, and managed a team of enumerators to conduct interviews based on electronic close-ended questionnaires.

To ensure as representative a sample as possible, the survey employed three levels of randomization. Thirty-four survey clusters were drawn in a two-stage process. In the absence of systematic and reliable veteran population data, up-to date data of the total population were used from the ongoing Angolan census. At each survey site we engaged with village chiefs, community coordinators and local administrations to produce listings of all former soldiers residing in the enumeration area (EA). Results were cross- and double-checked to develop credibly complete listings of the local veteran population. The sample was stratified by rural/urban areas according to aggregate census estimates. Conditional on the reliability of the general population as a proxy for the ex-combatant population, the sampling strategy is self-weighting and ensures that the geographic spread of clusters across the province is representative. If, as we assume, the population lists at the EA-level were complete, the EA-level sample is representative of the EA-level veteran population.

Sampled veterans had to complete two interviews. First, a private household-level interview, together with their (main) cohabitant partner or alone, in the case of veterans without a partner. Second, a private individual interview which included extensive modules on pre-military service, military service and war experiences, as well as post-war behavior and other outcomes. Sampled veterans' (main) cohabitant partners were interviewed simultaneously and privately, with a focus on intra-household and family outcomes.

Three first order threats to the validity of the empirical analysis are related to sampling and data collection: recall bias, survivor bias and other forms of bias from non-representative samples. A brief discussion for the implications for the comparative analysis of UNITA and MPLA soldiers will be offered here, while more elaborate discussion will be presented in the subsequent chapters.

[2] DW's focus is on physical infrastructure projects and has done very little work with former soldiers or on topics related to domestic violence or participatory governance. This was important to ensure their presence would not have confounding effects on the survey.

Recall bias poses a threat to any study using retrospective data, and the dominant source of error is misreporting due to memory failure. The main concern with memory failure is that respondents may not remember well because they need to recall situations that happened a long time ago or they did not perceive as intense or important. Individuals may also misreport for personal reasons, which is especially relevant for sensitive and traumatic experiences. Yet for misreporting regarding pre-military service data to actually introduce significant bias in the estimates of UNITA vs MPLA comparisons, the error would have to be systematic, i.e. asymmetric, across rebel and government soldiers. Given the striking balance across pre-service characteristics the thesis will find, it seems unlikely that such a systematic difference in misreporting would be group-specific.

Another concern is survivor bias, which is a natural limitation of conflict-related surveys as potential respondents who died can obviously not be sampled. This concern emerges if non-survivors' distribution of pre-service or service outcomes, or a characteristic correlated with any of these, is systematically different from those of survivors. A simple example is that soldiers from poorer families were more likely to die in combat. Yet again, for our comparative results to be substantially affected, would require a significant asymmetry across the two armed groups. This could be the case if, for instance, one group specifically targeted poorer people more than the other group. While plausible, the empirical findings will point away from this concern. Again, the marked balance across pre-service covariates, the absence of any strong conditional selection based on these, and the stability and symmetry of the age distribution the thesis will report, present strong arguments against such an asymmetry and concern.

Perhaps the most serious concern in practice are other forms of a non-representative sample. In the absence of official local veteran population lists the degree of representativeness hinges on the quality of the local sampling frames we built with local authorities. These frames may be incomplete and not representative. Local authorities may possibly favor those who served in the same faction they served themselves or favor MPLA veterans, as many village chiefs, for instance, receive regular, small compensations from formal MPLA authorities. During the survey we made a great effort to work with *all* relevant local actors and authorities we could identify, to provide enough time for them to produce reliable population lists, and to then cross-validate lists from different sources. Wherever applicable, these sources explicitly included civil society groups affiliated or associated with UNITA and local branches of the MPLA and UNITA political parties. Not surprisingly, also non-partisan sources, such as the village chiefs, which usually were the principal source of information, would consistently have accurate knowledge who was with an armed group during the war, allowing them to produce reliable combatant lists. One perhaps surprising observation from preparatory fieldwork is worth noting in this regard. In the preparatory phase it had been considered to stratify the sample by former faction. However, local informants would very often fail to produce separate lists for UNITA and MPLA veterans. This observation further mitigates concerns that local informants would systematically exclude

certain populations.

Based on these insights and the presented arguments regarding asymmetry for non-survivors, it is difficult to imagine why and how the group left out may be different in a way that would meaningfully affect the results.

1.3.6 Scope

The thesis will argue that the discussed effects are internally valid, including that the mechanisms linking conflict experiences to post-conflict behavior have a causal and generalizable interpretation. Yet in what sense will the results be relevant beyond Angola? The nature of conflict actors and military service experiences around the world suggest that the treatments and treatment effects are relevant to a diverse set of other conflict zones.

First, a growing number of armed actors have now been documented to provide public goods and social services to local populations in similar ways as the Angolan Civil war actors did. Beyond state actors, this list includes non-state actors as diverse as the successful *Eritrean People's Liberation Front* (EPLF) insurgency, the long-term *Revolutionary Armed Forces of Colombia* (FARC) guerrilla movement, the transnational *Islamic State of Iraq and Syria* (ISIS), or bandits in stateless areas in Eastern Congo. Levels of perpetrated sexual violence vary substantially across armed groups and the systematic factors underlying this variation are not well-understood. Yet, the moderate and rather selective use of sexual violence by the Angolan factions are consistent with predictions from existing models, based on a comparably weak form of forced recruitment and a certain degree of internal cohesion. The Sexual Violence in Armed Conflict dataset (Cohen and Nordas 2016), lists a number of longer conflicts where both state forces and a rebel actor committed similar levels as in Angola. These cases again include diverse conflict settings, such as Burma, Nepal, Peru, Sri Lanka or Colombia. A non-state actor in a historically close case with comparable levels of sexual abuse was the *Resistência Nacional Moçambicana* (RENAMO) in Mozambique.

Second, a coercive nature of military service is very common among state actors but also includes non-state actors. Forms for these range from abduction as by Sierra Leone's *Revolutionary United Front* (RUF) or the *Sudan People's Liberation Army* (SPLA) to weak forms of coercion as by the insurgent groups in the Guatemalan Civil War. Causal effects of forced recruitment at a young age are available mostly for two sets of cases that compare forced recruits to non-veterans: The draft set-ups by strong states, as in the US Vietnam service, and abduction in Northern Uganda. Even when the nature of recruitment may be roughly comparable, the post-service socio-economic environments are, of course, very different across contexts. Also, this project studies the effects of experiences related to forced recruitment as opposed to forced recruit vs. civilian comparisons. Generally, however, the results in this thesis are broadly

consistent with findings from these contexts in that forced service at a young age may have significant effects on recruits' behavior and welfare many years after service ended (Angrist 1990, Blattman and Annan 2010).

Third, on a more speculative note, compared to contexts where participation in armed groups is not coercive the Angolan findings may understate negative long-run impacts of military service experiences in armed conflict. Positive impacts, on the other hand, as on participation in public good production, may be overestimated. The reason is that when service was voluntary or the post-war society is socially less cohesive as in Angola, veterans will likely face significantly more issues and higher risks in the post-war period. This could exacerbate or at least not mitigate negative impacts caused by service experiences, and dampen positive effects. If this is the case, the size of the estimated effects in this dissertation may then be regarded as a lower bound for negative effects, and an upper bound for positive effects.

1.4 Findings

Chapter 2 will develop a simple theory of rebel recruitment motivated by the literatures on rebel governance and state capacity. The theory argues that a non-state actor may have the institutional capacity to choose and enact "state-like" recruitment, which is primarily based on tax compliance rather than individual motivation or an act of physical violence. Integrating state logic, the theory further predicts that an actor that has the ability to tax, and seeks to build a large army or engage in large-scale belligerent activity, will prefer and use conscription.

Traditional theories of rebel recruitment are motivation-centric, while conscription is age-centric, which predicts observable differences in background profiles between UNITA rebel and (conscripted) MPLA government recruits. The presented theory is not motivation-centric, and predicts no differences.

The empirical results support the theory's predictions: the background profiles and recruitment patterns of UNITA recruits are indistinguishable from those of MPLA recruits. The results instead suggest that army assignment for young men in the Angolan War was strongly predicted by local territorial control. It will be argued that UNITA was a) able to use, and also b) chose tax-based recruitment in the form of conscription in practice. The results challenge conventional theories of rebel recruitment and emphasize the crucial role of institutional capacity for rebel tactics.

Chapter 3 will argue that, theoretically, it is a priori unclear whether the exposure to armed group governance increases or decreases participation in collective public good production in the long run. It will propose and test four complementary theoretical mechanisms: 1) balance of trade mechanisms based on repeated economic interactions with civilians, 2) a reduction of in-group bias against civilians based on repeated social interactions with civilians, 3) social learning of

cooperation, and 4) a shift in political preferences.

The empirical analysis will present causal evidence that former soldiers who were more involved in local governance by armed groups during the war are more likely to participate in public good production more than a decade after the end of the war. It will be maintained that these effects are not driven by local effects, omitted individual variables, measurement error, mis-specified intercorrelations in the error term, inferior index measures of group governance, and responses to a singular question in the index.

It will be maintained that the positive effect on participation in public good production is based on gaining experience with social cooperation and a shift in political preferences. Exposure to armed group governance does not foster mobilization in a wider political sense, as in the form of voting in presidential elections or regional protesting, and does not increase cooperation within the family. These results will be interpreted as evidence that exposure to wartime governance may stimulate lasting interests and participation in local politics, governance, and collective action, which has important implications for local institutions, development and state-building in post-conflict societies.

Chapter 4 will maintain that, theoretically, it is a priori also unclear whether the exposure to sexual violence against civilian women increases or decreases males' propensity to commit domestic violence in the long run. It will introduce and test five complementary theoretical mechanisms. The exposure to wartime sexual violence may 1) weaken a veteran's economic bargaining power, 2) create or exacerbate gender stereotypes, 3) creating violent models that soldiers learn, 4) condition soldiers into 'gender-based violent practice', by breaking a psychological barrier, or 5)cause long-term distress.

The empirical analysis will present causal evidence that the exposure to armed group sexual violence significantly increases a former soldier's propensity to commit domestic violence more than a decade after the end of the war. It will be maintained that this effect is not driven by local effects, omitted individual variables, measurement error, linear model specification, mis-specified intercorrelations in the error term or the choice of our preferred instrumental variable specification.

It will be explained that the increase in perpetrating domestic violence is caused by a process of conditioning into gender-based violent practice, and is psychological and persistent in nature. The results challenge dominant interpretations of domestic violence as a function of intra-family power structure or prevailing cultural norms. Instead, the paper supports an interpretation of domestic violence as expressive behavior that is not triggered by payoff-relevant considerations and introduces the argument that such behavior may have long-run origins.

1.5 Related literature

The thesis makes important contributions to understanding the origins of differences in human behavior and the causes and consequences of violent conflict. In doing so, it relates to a number of fields and literatures as outlined very briefly below. Some of these will be discussed in more detail in Chapters 2 to 4.

First, the thesis uncovers novel origins of differences in human behavior. Microeconomic theory, based on the work of Becker (1962), emphasizes the role of 'human capital', defined as a set of skills and traits, including health, that contribute to a worker's labor productivity. Here, human capital may be manipulated by interactions with state institutions or private organizations ('investments') through services such as schooling and training. More recent economic views re-define 'human capital' to include broader capabilities of being a productive member of society, such as functioning reliably in organizations and obeying orders. More generally, recent research advances that some fundamental assumptions of the standard micro-economic model may need to be updated. This includes the previously neglected role of culture and norms in shaping beliefs that govern behavior, as well as the observation that innate preferences may be malleable over the life course.

Then, what is the role of wartime military service at a young age in shaping behavior? Military studies provide a wealth of evidence how - belligerent and non-belligerent - military actors train and form their soldiers in ways that foster traditional labor market skills. Studies in political science and public economics have shown that the exposure to violence may make individuals more cooperative and politically engaged. On the other hand, labor economists have emphasized that forced military service may create human capital losses due to time away from formal schooling and the labor market. Health economists have documented the psychological and physiological health risks of military service in war zones.

As recent research in identity economics shows, both state and non-state military institutions often also inculcate culture and norms deliberately. Studies in behavioral economics argue that fundamental preferences respond mostly between ages 18 and 25, while a different set of studies suggests that preferences may respond to the exposure of war and violence.

The thesis builds on these results and offers novel theoretical and empirical insights into the long-run origins of social and political behavior, which is causally coded by differing exposure to conflict policies and practices.

Second, the thesis advances the conceptual understanding and analysis of armed conflict. The study of armed conflict and the effects of armed conflict from a micro-perspective is limited by scarcities in three elements: high quality micro-data measuring 'exposure to conflict', causal identification strategies estimating the long-run effects of exposure to conflict, and theoretical mechanisms underpinning

these (Blattman and Miguel, 2010). A substantial share of these limitations can be attributed to two factors. First, collecting data in contexts where institutions are weak and violence is pervasive, presents a daunting task. Second, existing quantitative studies often lack deep qualitative knowledge of the local context and its history as – among other factors – time in the field is often very costly and collaborative projects are still not common.

This study directly targeted some of these concerns by partnering with an anthropological study and an Angolan NGO. In combination with innovative quantitative techniques, this approach ensured to reflect priorities of respondents and leverage an originality of the context, yet produce quantitative findings that have a causal interpretation and derive recommendations that are applicable to other contexts. Specifically, this study provides original data to describe novel aspects of the exposure to conflict and helps unpack the treatment bundle military service entails, novel theoretical mechanisms to understand the lasting effects of conflict, as well as unique causal strategies to identify and quantify these long-run effects.

1.6 Policy implications and conclusions

The findings presented in this thesis are relevant to assistance policy by international donors and national agencies in post-conflict contexts. In such contexts, veterans' post-war trajectories are key elements in the transformation of a conflict-affected to a peaceful state, where former fighters are often considered a primary threat to political stability, social cohesion and economic development. They are therefore a focal point of large-scale development assistance, as in participatory and community-driven development (CDD) and demobilization, disarmament and reintegration programs (DDR). Yet, implemented projects struggle to deliver an impact, and individuals' and communities' responses are far from uniform, underscoring the fact that designing these interventions is a demanding challenge.

The results from this study can help understand why individuals and communities may respond differently to certain post-war interventions. On the one hand, a key finding is that individual experiences during a conflict may vary substantially, even for individuals of very similar backgrounds. Choices at the group level, plausibly beyond an individual's control, are found to play a significant role in shaping heterogeneous experiences. On the other hand, the findings also emphasize that these differences may create lasting and systematic differences in outcomes directly affecting social cooperation, political stability and human development. That military service may actually generate rather than level out behavioral differences among individuals may be unexpected from a military socialization perspective and cautions against one-size-fits-all approaches to post-conflict policy.

Notably, the detected lasting legacies are diverse in nature. First, the findings document positive as well as negative long-run effects: the findings suggest a causal

effect that makes individuals more likely to participate in local politics and collective action, but yet another mechanism fosters instability and violence in the post-conflict family. Second, a deeper look into these effects suggests that some are based on psychological scars and socialization mechanisms, while others are caused by a shift in political preferences.

Taken together these insights suggest that post-conflict assistance must try to take the lasting effects of conflict into account, which will allow to build better interventions, more tailored to those having experienced violent conflict first hand. Ultimately, to be able to understand, account for, but also leverage *more* of the variation in post-conflict outcomes and their origins, more high-quality conflict data will be needed, from more social groups, including women and civilians. Similarly, more well-theorized and causally-identified micro-studies will be required to produce more rigorous evidence based on these data. As this study confirms, interdisciplinary approaches and profound historical and ethnographic understanding of the context may be a promising strategy to overcome some of the existing methodological obstacles to progress.

1.7 Statement of project and conjoint work

I certify that this thesis is the quantitative component of a joint research project funded by two grants from the United States Institute of Peace and the Portuguese Research Council. The qualitative component is a doctoral dissertation in anthropology by John Spall, and the research project was led and supervised by Tilman Brück. The project advisors included Patricia Justino, Pedro Vicente and Nikolaus Wolf.

I certify that Chapter 3 is based on joint work with Patricia Justino, and that Chapter 4 is based on joint work with Tilman Brück.

Chapter 2

Rebel without a Cause? Non-state Recruitment and Institutional Capacity

2.1 Introduction

Armed non-state groups are a fundamental threat to national and international security across the world. It is therefore important to understand how non-state actors form and grow, especially those that mobilize large numbers of recruits.

A vast literature has now studied motives of and mobilization for *voluntary* participation in rebellion and terrorism (Blattman and Miguel, 2010). However, between 1980 and 2009, 45% of insurgent groups worldwide used *coercion* to recruit members (Cohen and Nordås, 2014), and much less is known about these processes. Abduction – the forcible taking away of a person against their will – accounts for about one third of cases of coerced recruitment. The remaining two thirds of cases, however, are not well understood (Cohen, 2013).

By contrast, coerced recruitment by *state* actors has been studied thoroughly. A large body of both positive and normative studies documents and analyzes states' institutional abilities, and how these allow to enforce compulsory military service(e.g. Poutvaara and Wagener, 2011). Alternative strategies for mobilizing and sustaining big armed forces include volunteering and (for weak states) abduction, but both are considered as inferior in the medium- or long-term, due to the associated economic and political costs (Ross, 1994; Mulligan and Shleifer, 2005).[1]

Institutional characteristics are surprisingly absent in theories of recruitment and large-scale mobilization by non-state actors, even though it is well-established that

[1] Beber and Blattman (2013) present similar arguments for abduction of children by rebel actors.

institutional factors are a central determinant of non-state actors' policy choices, such as battlefield tactics (Bueno De Mesquita, 2013; Wright, 2016). This paper studies military recruitment by non-state actors of high 'institutional capacity', defined as a general equivalent of state capacity – a ruling actor's capability to choose and implement 'state-like' policies, such as the provision of security, the delivery of social services and taxation. The paper advances two arguments. First, a non-state actor may have the institutional *capacity* to enforce 'state-like' recruitment policies, which essentially rely on individuals' tax compliance and are not primarily based on their intrinsic motivation or the use of physical force against them.[2] Second, among such policies, a high-capacity non-state actor's preferred *choice* will be equivalent in nature to conscription.

Strong states staff their armies either by conscription or by hiring volunteers at market wages.[3] Both methods rely ultimately on the state's extractive capacity, as the state either levies an in-kind tax in the form of inductees' forced service (conscription), or uses tax revenues to compensate hired soldiers (in a professional army). Comparing the two forms, the empirical literature has noted two stylized facts. First, large state armies are (still) overwhelmingly built by conscription, consistent with general theoretical arguments (Poutvaara and Wagener, 2011). The economic intuition is that paying each member of a large army a market wage requires a level of taxation that would impose enormous deadweight costs on the economy (Ross, 1994).[4] Second, state actors engaged in belligerent activity conscript more often than those that are not (Pickering, 2011).

Based on data from 2007, Figure 2.1 presents a world map of states with conscription laws in place and demonstrates the global prevalence of conscription (of young men).[5] While there is a recent trend among wealthy states to favor and switch to volunteer armies, most countries reserve fallback options for crises and wars. Ukraine is a recent example of such a reversal to compulsory service in practice. Another example for the relevance of conscription in strong modern states is that in the 2000s both Republican and Democrat lawmakers in the United States introduced bills for reinstating a military draft (e.g. CD, 2006).

Armed non-state actors have traditionally been associated with low institutional capacity. As an example, the dominant view in the conflict literature conceptualizes

[2]We will assume that tax compliance is enforced without the use of actual force, as in strong states. When ruling actors lack 'capacity', this will usually not be true, for both state and non-state actors. These issues will be discussed in more detail below.

[3]Conscription includes lottery types, such as the US draft or the UK call-up, and often co-exists with volunteering. Moreover, we focus on 'strong' states in this paragraph. As we will discuss in Section 2.2, weak and fragile states also use stronger forms of forcible recruitment, primarily a method called 'press-ganging' (Cohen, 2013).

[4]A less considered short-run alternative is enormous wealth, which we will discuss later.

[5]In theory, there are universal forms of conscription. In practice, the legal obligation to serve is typically confined to a certain sub-group of the population – usually young able-bodied males. The practicability of compulsory service for sub-groups is consistent with the argument that individuals are more likely to contribute to forms of collective action, as in the case of national defense, when they believe that all in their sub-group will contribute (Fischbacher, Gächter and Fehr, 2001).

insurgency – the most dominant type of armed conflict in the post-World War II era – as "a technology of military conflict characterized by small, lightly armed bands practicing guerrilla warfare from rural base areas" (Fearon and Laitin, 2003, 75). Yet the nascent literature on 'rebel governance' demonstrates that non-state actors often build stable and coercive institutions of governance in situations where state sovereignty and territorial control are contested or absent (Berman and Matanock, 2015; Arjona, Kasfir and Mampilly, 2015). Notably, some actors have sufficient institutional capacity to establish local, legitimate monopolies of violence and enact policies to sustain markets and raise revenue (e.g. Sánchez de la Sierra, 2015). This suggests that these actors have a certain degree of coercive power, which they may use for recruitment policy, i.e. to build recruitment institutions and enforce the policy they choose.

This paper hence argues that such a non-state actor may have the institutional capacity to choose and enforce 'state-like' recruitment, which is primarily based on tax compliance rather than individual motivation or an act of physical violence. Integrating state logic, the paper predicts that an actor that *has* the ability to tax and seeks to build a large army or engage in large-scale belligerent activity, will prefer and use conscription.[6]

To assess the explanatory power of these arguments empirically, we collected survey data from 760 government and rebel veterans of the Angolan Civil War, as part of the Study of Angolan Ex-Combatants project (POEMA). The dataset contains detailed information on military service experiences, pre-service background characteristics and recruitment circumstances.

Angola's recent history offers a suitable setting for our study. The 27-year war (from 1975 to 2002) presents a long, intense and dynamic case of mass militarization and high-capacity military competition between the *Movimento Popular de Libertação de Angola (MPLA)* government and the *União Nacional para a Independência Total de Angola (UNITA)* rebels. The MPLA government introduced conscription for men aged 19 to 45 in 1976 (Junior, 2015). Importantly, the Angola literature describes in great qualitative detail that the UNITA rebels in parallel built a 'state in a state', which included a large military apparatus and the systematic extraction of goods and services from the population, including labor (Pearce, 2012; Roque, 2015). This configuration thus suggests two things. At the armed group level, we expect to be able to study recruitment by a non-state actor with high institutional capacity, which can be contrasted with conscription by state actor. At the individual level, we expect the total Angolan population of former soldiers to be large, and to contain substantial variation to study and test recruitment processes from a micro-perspective.

The empirical analysis proceeds in three steps. The preliminary first step examines aggregate individual-level data on region and date of recruitment as well as

[6] As already noted above, state actors in weak and fragile contexts also use stronger forms of forcible recruitment, beyond and sometimes in addition to conscription, which likely also applies to non-state actors. These scenarios will be discussed in Section 2.2.

involvement in wartime interactions with civilians during military service, to provide micro-based assessments of territorial control, institutional capacity and policy choices. Second, the main analysis compares recruits' pre-service background profiles, to test whether UNITA recruits were systematically *different* from MPLA recruits along these lines. The analysis will implicitly assume that MPLA did employ age-centric conscription in practice (as prescribed by law, in theory). Traditional theories of rebel recruitment, which are based on identity (e.g. Humphreys and Weinstein, 2008), would hence predict systematic differences. Third, we relax the strict conscription assumption on MPLA and explore descriptively to what degree MPLA and UNITA's policies and practices were consistent with (de-facto) conscription and conscript armies. We will focus on UNITA and try to distinguish conscription from recruitment based on physical force, such as abduction, and hiring professionals. To do so, we include auxiliary micro-data from abducted soldiers in Northern Uganda for comparison (Blattman and Annan, 2010), analyze survey data on material rewards and sexual violence by armed groups against civilians, and review qualitative and anecdotal evidence.[7]

From the aggregate data on recruitment and exposure to wartime policies, we find that many soldiers on both sides help to implement policies during the war that require – and therefore reflect – substantial institutional capacity. Specifically, the data indicate a certain degree of ability to tax by UNITA, in keeping with historiographic and ethnographic accounts of the war. The descriptive findings also corroborate existing evidence suggesting that territorial control, which can also be thought of contributing to capacity, shifted often and substantially.

Second, the data are not consistent with identity-based, voluntary recruitment into rebel groups. Among other factors, poor, uneducated and protestant young men were just as likely to have joined MPLA's army as the UNITA rebel alternative. We do not find systematic unconditional differences in any item of their demographic profiles. The results hold for conditional mean comparisons from multiple regression models, the inclusion of recruitment date and location fixed effects, clustering standard errors in these dimensions, Bayesian model averaging analysis, and predicted probabilities from non-linear models. While we detect no temporally consistent selection along regional lines and over time, we show that the *interaction* of recruitment date and location is a strong and robust predictor of the army assignment, which is consistent with our previous findings on the central role of territorial control for recruitment.

Third, the data also provide suggestive evidence consistent with conscription on either side. For both armies, the recruitment age is normally distributed, with a lot of probability mass in late teenage years. These patterns are stable over time, i.e. over dates of recruitment, and resemble standard patterns of conscription, where the main determinant for date of entry is an individual's age (among the eligible sub-group, which are usually able-bodied men). While UNITA recruits were slightly younger on average, the distributions for both armies are clearly different from abducted soldiers

[7]The analysis of wartime sexual violence is motivated by the theory of (Cohen, 2013) which posits that abducted armies are more likely to rape, in order to increase internal cohesion.

in Northern Uganda, which were significantly younger at the time of recruitment, on average. UNITA soldiers were not significantly more likely to be exposed to wartime sexual violence and were significantly *less* likely to receive monetary or other rewards on regular basis. While only scattered qualitative and anecdotal evidence on UNITA recruitment exists, these accounts confirm that UNITA recruitment was coercive, in the form of conscription, possibly mixed with forced abduction at certain times.

Taken together, the evidence suggests that UNITA a) was able to use, and b) did use tax-based recruitment in the form of conscription in practice, supporting the theoretical predictions. Other forms of recruitment likely occurred in addition, which would not be surprising but rather expected, as recruitment rarely relies on strictly one strategy.[8] We interpret these results as evidence that conscription was UNITA's preferred strategy, and speculate that stronger forms of coercion and physical force were used when it temporarily either lacked the institutional capacity to organize conscription or enforce it nonviolently.

Three natural threats to the validity of the empirical approach and results are recall bias, survivor bias and other forms of bias from potential non-representativeness. We argue that it is difficult to imagine how and why any of these would introduce asymmetric distortions in the recruitment profiles across the two factions. [9]

The theoretical arguments and empirical findings of the paper contribute to three literatures. First, the paper extends the literature on rebel and terror recruitment, which has focused on voluntary participation and abduction (Blattman and Miguel, 2010; Berman and Matanock, 2015). This paper studies a form of coercive recruitment that is 'weaker' than abduction and not primarily based on an act of physical violence. Theoretically, it emphasizes institutional capacity as an explanatory variable of recruitment by non-state actors, and adds the argument that a non-state actor may have access to tax-based recruitment and, if so, will have a preference for conscription. In addition, the empirical findings are consistent with the theoretical predictions and provide evidence *against* large-scale participation based on volunteering or abduction.

Second, the paper complements the literature on state recruitment, mass mobilization and conscription. This literature has focused on questions of when and why strong states use conscription (Levi, 1989; Mulligan and Shleifer, 2005; Poutvaara and Wagener, 2007; Konstantinidis, 2011) and what the long-term costs at the individual level are (Angrist, 1990; Imbens and van der Klaauw, 1995). This paper offers theoretical and empirical arguments that a *non-state actor* may be able to mobilize en masse 'like a state', using conscription. A minor contribution is that the paper also provides suggestive evidence of recruitment by a *weak* state actor.

[8] For instance, a conscript army in strong states will always accept and insert volunteers (Mulligan and Shleifer, 2005), as noted above.

[9] Section 2.5 discusses these issues in more detail.

Third, the paper adds to the emerging literatures on conflict governance and strategies by non-state actors. Studies of rebel strategies have focused on fighting tactics (Bueno De Mesquita, 2013; Wright, 2016), while the nascent 'rebel governance' literature has concentrated on an actor's capacity to choose and enact state-like policies as public good provision and taxation (Arjona, Kasfir and Mampilly, 2015). This paper integrates insights from both streams of literature to study rebel recruitment as a function of institutional capacity.

The remainder of the paper is organized as follows. Section 2.2 develops the theoretical arguments on non-state recruitment based on the foundations of institutional capacity. Section 2.3 discusses the Angolan context and why it is an appropriate setting to test the theoretical arguments empirically. Section 2.4 describes the data and empirical design. Section 2.5 presents the main empirical results. Section 2.6 discusses the mechanisms underlying these results. Section 2.7 reports robustness tests and compares the findings to available qualitative and anecdotal evidence. Section 2.8 concludes.

2.2 Recruitment and institutional capacity

2.2.1 State actors and recruitment

Background. All big international wars of the 21st century were primarily fought between strong states and their mass armies built with conscription. Normative questions related to the efficiency and equity of conscription are controversial and dominated by American perspectives. Especially the high fatality rate during the Vietnam War sparked controversies about the draft in the United States and led to its eventual elimination in 1973. Most academic studies tend to argue against conscription in favor of an all-volunteer force, based on grounds of costs and benefits in general equilibrium (e.g. Warner and Asch, 2001).[10]

Historically, states have employed four methods to raise armies: conscription (including lotteries), hiring in the market, substitution, and commutation (Levi, 1989). Conscription is usually universal, which, as noted above, may be confined to a demographic subgroup and/or random draws, as in the United States Draft or the United Kingdom Call-up. It legally obliges selected individuals to perform military service, with strict rules for exemptions and non-compliance. Despite the terminology, conscript armies are often a mixture of professional and drafted forces, as volunteering is usually not prohibited. Hence, most conscript armies will not rely solely on conscripts but will usually have some proportion of volunteers (Mulligan and Shleifer, 2005). Substitution and commutation have disappeared in the 19th century (Levi, 1989).[11]

[10] For a differing view, see e.g. Lee and McKenzie (1992).

[11] Both options share the legal obligations of conscription but include more room for exemptions.

The role of state capacity. All forms of recruitment used by strong, modern states – conscription or hiring volunteers – rest on the state's ability to tax its constituency. Professional state armies hire soldiers in the market and the state compensates them for their opportunity costs of service through tax revenues. Conscription can be similarly interpreted as a state tax on the conscripted by converting the fiscal burden of defense into in-kind contributions (Poutvaara and Wagener, 2011). To organize the main argument of the paper, we now distill the foundations of the state's ability to tax and conscript.

Understanding why individuals contribute private resources, such as part of their income, towards a common good, such as security and defense, is closely related to the more fundamental question of what 'the state' itself is. Conceptually, taxation is often considered as one of the 'foundational policy bargains' of the modern state (Konstantinidis, 2011). Voluntaristic theories essentially view states as the result of a mutually beneficial 'social contract' between a ruling actor and its constituency (Rousseau, 2001) and individuals' obligations are hence predicated on this social contract. An alternative and recently popularized interpretation views the state as the result of successful 'organized crime' (Tilly, 1985; Olson, 1993). The organized crime perspective emphasizes the origins of the state in conflict and the importance of establishing a territorial monopoly of violence (Weber, 2009).[12] Crucially, in this interpretation individuals' (tax) contributions are rather coerced than given by consent. It is hence (also) a matter of perspective whether compliance with a tax policy – as conscription – is fundamentally based (more) on coercion or consent.

Modern political economics teaches that enacting a tax policy hinges on a state's 'capacity', broadly defined as the ability of the incumbent government to choose and implement public policies (Besley and Persson, 2010). Among others, Mann (1984) divorces the state's *political authority* from its bureaucratic or *administrative capacity* to develop, finance, carry out and enforce policies. Together they endow the state with the 'ability to tax'. Effective conscription thus requires a minimum level of a) capability to reach the population, b) infrastructure to design and implement such a policy, and c) ability to ensure and sustain popular compliance with the policy. While a) and b) are primarily examples of administrative capacity, c) highlights the fundamental role of authority for an effective conscription policy, and taxation more generally.[13]

The nature of how exactly state capacity achieves compliance with authority and policies is subject to debate, and is related to the question presented above on whether the state itself is (more) built on consent or coercion. A dominant explanation, however, is 'legitimacy'. The role of legitimacy in the Weberian approach to the state is contested as well, but Weber himself defined that legitimacy

Substitution allows for a willing replacement to serve in the place of a drawn draftee, usually in exchange for a replacement fee, while commutation concedes draftees to buy themselves out.

[12]The general organized crime approach is not limited to state actors, and has also been applied to rebellion (e.g. Collier, 2000).

[13]Especially in 'high-capacity states', ensuring compliance generally relies strongly on administrative capacity, such as a complex system for monitoring tax compliance, and the legal capacity for enforcement (Besley and Persson, 2009).

manifests itself in the 'disposition to obey authority' (Weber, 1994). Individuals with such a disposition will accept the authority's legitimacy as sufficient reason for compliance (Raz, 1986). Whether or not we view compliance based on legitimacy as coercion, ruling state actors may also have the capability to use (stronger forms of) coercion to ensure compliance (e.g. Blair, 2015). So, when is strong coercion a viable option? Political scientists argue that (stronger) coercion is costly and limited in success to the short-term, and practical only when needing to target 'relatively few' while 'the many' do not need to be coerced (Deutsch, 1981). This hence suggests that sustained government is unlikely in the absence of a certain degree of *legitimacy*, even if the actor's institutional capacity is generally high.

Alternative strategies. The discussion above assumes strong states, which meet the capacity requirements for conscription just described. As we will compare non-state recruitment to that of a civil war state actor, it is important to recognize that a state actor engaged in internal conflict lacks a certain level of state capacity by definition. In addition, state actors that have the ability to tax may opt to choose other policies for a certain reason that outweighs the incentives for using tax-based forms of recruitment. While only very few systematic studies exist, the literature has documented 'press-ganging' as a third form of recruitment into state armed forces (e.g. Cohen, 2013).

Press-ganging means that individuals are kidnapped into service in state forces without notice, which may include children (Brett, McCallin and O'Shea, 1998). Cohen (2013, 468) estimates that between 1980 and 2009 close to one third of state forces engaged in modern civil wars used press-ganging and offers two illustrative examples. First, a case from Nicaragua: *"The Sandanista Army continued military impressment, conducting sweeps of public facilities and forcibly removing youths as young as 12."* Second, a case from Ethiopia: *"Although a military service decree was issued and youth are being required to register, the authorities still frequently round up youth off the streets or seize them from their homes to press them into military service."* Press-ganging is hence clearly a stronger form of forced recruitment than conscription.

Summary. To summarize, we have established that effective conscription is predicated on the institutional capacity of the state to tax its populace, as is the main alternative – a professional army. Sustained government and effective policy enforcement is unlikely in the absence of a certain share of legitimacy in overall capacity. For states that lack capacity or have other reasons for doing so, the literature has observed press-ganging as a third practice of recruitment. As discussed in the introduction, states that have the ability to tax and seek an efficient allocation of labor, will prefer conscription over professional armies when they build a large army and/or are at war.

2.2.2 Non-state actors and recruitment

We now analyze non-state actors from an institutional capacity perspective and apply the insights we just derived for state actors.

Non-state actors in conflict across the world establish stable local monopolies of violence, invest in relations with the governed populace, and extract goods, labor and services in territory under their control (Arjona, Kasfir and Mampilly, 2015). Policies of provision include external protection, internal security, health care, education, schemes of conflict-resolution, political councils, roads, or land reforms. Extractive policies include regulating local markets and taxing labor, crop yields, food sales, or transit of persons (see e.g. Sánchez de la Sierra, 2015). In the terminology adopted here, these policies reflect institutional capacity, and the 'ability to tax' the governed population.

In light of comparing non-state actors to state actors, it is worth noting in this regard that two different and important roles of public good provision have been advanced. First, similar to states, non-state actors may use it to enhance local productivity and therefore increase taxation revenues later (Sánchez de la Sierra, 2015; Besley and Persson, 2011). Second, again similar to state actors, public goods are also an efficient way for non-state actors to build legitimacy (Levi, 1989; Stewart, 2016). Thus, non-state rulers with a certain level of capacity may make credible, legitimate claims to sovereignty, authority and statehood in occupied territory. Applying state logic, this suggests the possibility of sustainable government and effective policy enforcement.

While other tax policies are now well documented, not much systematic knowledge exists about recruitment policy under 'non-state governance'. Some existing arguments further emphasize the importance of territorial control and sovereignty – which in our framework can be thought of as forms of institutional capacity – for recruitment policy. Kalyvas (2006) argues that, when armed organizations control a locality, the costs of collaboration between the actor and the populace of the locality may go down for both of them. This confirms intuitive arguments that controlling a given locality is conducive to recruitment of people living in the locality. From a different perspective, Fearon and Laitin (2003) advance the abstract argument that low levels of capacity by a state actor encourages the emergence and development of rebellion against this actor. Applying this logic to the local level then suggests a similar conjecture as above: a non-state actor claiming control and sovereignty, should be more likely to grow, if only by more 'access' to people.

The basic theoretical proposition is hence that a non-state actor may have the capacity to choose and enact a coercive recruitment policy that is primarily based on tax compliance rather than physical force by the armed actor. Based on state logic, this includes two types of policies: conscription and a tax-funded army of volunteers. Applying more insights from states actors, the second theoretical proposition that

follows is that a non-state actor that has the ability to tax, will prefer conscription for building a large army, and/or when engaged in high levels of belligerent activity.

Alternative strategies. Just like in the case of state actors, it is important to recognize that a high-capacity non-state actor engaged in internal conflict may not *always* or *everywhere* have the ability to tax or opt to choose other policies for a certain reason. For non-state actors, the literature has documented forms of coercive recruitment that are just "short of abduction" (Cohen, 2013, 468). We consider these forms of coercive recruitment as the conceptual non-state counterpart of press-ganging by states discussed above. Cohen (2013, 468) provides an example from Guatemala: *"Guerrillas also committed human rights violations including [...] forced labor and recruitment."* This quote highlights a fundamental characteristic of this form of recruitment: it is usually associated with human rights violations and the actual use of physical force, and it is hence very close to press-ganging. For ease of exposition and because this study straddles the conceptual boundaries between state and non-state conflict actors, we will refer to this category as press-ganging, irrespective of the nature of the actor.

Empirically, it is difficult to isolate the hypothesized conscription mechanism for non-state actors from the tax-based alternative of hiring volunteers and recruitment based on the use of physical violence in the form of press-ganging and abduction. Below, we derive two indirect and testable predictions for non-state armies.

First, an army of professionals will be compensated more regularly than any other army (as soldiers were hired). Similar to state logic, for big armed groups this is not efficient and hence in general rather unlikely. Yet in contexts like Angola, where actors have access to natural resources, a professional army might be funded by non-tax funds such as revenues from trading natural resource.

Second, we build on two existing, stylized facts on abduction. One, abduction favors very young persons under the assumption that they are easier to intimidate, indoctrinate, and misinform than adults Beber and Blattman (2013). Two, abducting a person is generally not done en masse or by bloc and it is always based on an act of physical force (Cohen, 2013). Cohen (2013) therefore theorizes that abducted groups often have less internal cohesion and will thus exhibit higher levels of wartime sexual violence than armies of (mostly) volunteers or conscripts. The underlying logic is that wartime rape may serve as an organizational policy to foster internal cohesion. Based on these two insights on abduction, we expect members of a conscripted non-state army to be older and less likely to be exposed to wartime sexual violence by the group than members of groups built by other, stronger forms of forcible recruitment.

2.3 The Angolan Civil War and institutional capacity

History of the conflict. Between 1975 and 2002, two highly organized and capable military factions fiercely battled for territorial control and their respective nation-building missions in the Angolan Civil War. These were the *Movimento Popular de Libertação de Angola (MPLA)* and the *União Nacional para a Independência Total de Angola (UNITA)*. Both organizations emerged as nationalist movements opposing the Portuguese colonial rule in the war of independence between 1961 and 1974, alongside the *Frente Nacional para a Libertação de Angola (FNLA)*. By the time independence was consolidated in 1975, inter-movement fighting had erupted over controlling the new nation. FNLA was to fold away soon, while MPLA, led by upper-class 'assimilados', seized control of the capital Luanda and became 'the government'. UNITA presented itself as the 'party of *all* Angolans' and initially seized control of much of the Southern and Eastern territories and became 'the rebels'.

Like MPLA, UNITA was originally a Marxist guerrilla movement, whose leader Jonas Savimbi had been trained as a Maoist, but transformed itself rapidly into an armed actor led by narratives of freedom and democracy after independence (Parsons, 2006). Both parties managed to secure strong international allies and consistent access to natural resources. MPLA relied on assistance from Cuba, the Eastern bloc and oil revenues, while UNITA was backed by South Africa, the US and diamond trade (e.g. Guidolin and La Ferrara, 2007; Berman et al., 2016). Notably, the more recent Angola literature points out that even in the Cold War period the movements' narratives were not predominantly ideological, but rather shaped by a rhetoric of national identity and defense of the new nation against rule of the opponent (Pearce, 2012).[14] This Cold War 'proxy-war' lasted from 1975 until 1991, and was characterized by large-scale front-line fighting, including the biggest conventional battle of post-WWII Africa in Kuito Canavale in 1987. In 1991 a ceasefire was agreed in the Bicesse Accords.

After abortive elections in 1992, MPLA and UNITA returned to war, now without support by their Cold War allies. Extremely violent episodes and 'see-saw' battles ensued, only interrupted by a failed peace agreement in 1994 (the Lusaka Protocol). Despite large-scale peacekeeping efforts by the United States following the peace agreement, the situation remained fragile and scattered incidents of localized violence continued. In 1997, the UN Security Council, now with the support of the United States, decided to impose sanctions on UNITA, but only a year later full-scale fighting resumed. In February 2002, MPLA secured a clear and undisputed victory, when UNITA's leader Jonas Savimbi was assassinated in an ambush. Military operations ground to a halt abruptly and the Luena Memorandum of Understanding

[14] These perspectives stand in slight contrast to an early Angola literature and cross-country studies that classify the Angolan Civil War and its combatants sometimes in ethnic, religious and natural resource terms. In these accounts MPLA is related to the Mbundu ethnic group, urban *mestiços* with a Roman Catholic background and driven by prospects of oil revenues, and UNITA to rural Congregationalist Ovimbundu and profits from diamond trade (e.g. Marcum, 1989).

was signed in April 2002, which included rapid mass demobilization on both sides.

Military history. MPLA's armed forces were the *Forças Armadas pela Libertação de Angola* (FAPLA; *engl. Armed Forces for the Liberation of Angola*), while UNITA's army was called *Forças Armadas de Libertação de Angola* (FALA; *engl. Armed Forces of the Liberation of Angola*). Technically, these military institutions were separate from the political bodies, but for either organization the two bodies were compounded (Parsons, 2006), and for ease of exposition we refer to military service 'for MPLA' or 'for UNITA'.

In the wake of becoming the Angolan government in 1975, MPLA transformed FAPLA from a guerrilla army into the new Angolan state's armed forces. The reforms included that conscription into the FAPLA became compulsory for Angolan citizens by law 2/76 (Junior, 2015). The law specified that military service was obligatory for individuals between 1 January of the year they turned 20 and the 31 December of the year they turned 45. By law, military service was compulsory for either sex, but in practice hardly any women were recruited (Spall, 2015). This practice led to a case of mass militarization and mass recruitment of men from government-controlled territory throughout the country. In the early 1980s, FAPLA had grown an army of about 75,000 troops, in 1987 it reached an estimated size of about 120,000 (Weigert, 2011), and by 1992 several hundreds of thousands of men had passed through its ranks (Messiant, 2008).

Information on UNITA's army and recruitment is relatively scarce and less conclusive. Most existing knowledge is anecdotal, which will be discussed in Section 2.7. One aim of this article, beyond the generalizable objectives outlined above, is hence also to contribute descriptive evidence on the recruitment practices by UNITA. In terms of numbers, policy reports estimate that (in the formal process) after the end of the war in 2002 about 500,000 UNITA soldiers and family members had to be reintegrated (Parsons, 2004*a*). This strengthens the basic assumption that UNITA built and used a large military apparatus.

Relevance. Angola's recent war and military history offers a promising setting for this study for two reasons. First, immediately after independence, the nation experienced a prolonged case of mass militarization and intense warfare, where territorial control was highly volatile. Starting from Southern and Eastern regions, UNITA sought to gain territory to fight off MPLA, secure food and economic production, confer legitimacy and increase social support (Parsons, 2006). By 1984, UNITA had established itself in every province (James, 2011). As Pearce (2012) notes, the end of the war in 2002 marked the first time since independence that the government had at least notional control of the entire Angolan territory, including large areas it had not held in a long time or ever. At certain points during the war, the government held as little as 20% of its nominal territory. On the other hand, the war was fought by the same two actors throughout, which allows for 'clean' and

temporal comparisons of the two.

Second, both sides engaged frequently and often systematically with the respective local populations under their control. The literature on the Angolan Civil War portrays the competing factions as highly capable actors with refined strategies to pursue their national missions (e.g. Pearce, 2012). Specifically, the literature suggests that UNITA was a high-capacity organization that resembled a "state in the state": they claimed authority, legitimacy and a monopoly of violence in territory they held, regulated social and economic civilian life, delivered public goods and services, such as protection and infrastructure, and extracted food, labor and services (Parsons, 2006; Pearce, 2015*a*; Roque, 2015). Brinkman (2003) reports that favorable attitudes towards an occupying actor hinged directly on its ability to protect civilians and their land, i.e. some form of institutional capacity and delivery of services.

This configuration suggests that, at the group level, the Angolan context allows to study large-scale military recruitment and a high-capacity non-state actor. At the individual level, the configuration suggests that the populations of former government and rebel soldiers are very large and allow for valid and informative comparisons. Specifically, we expect substantial variation within both populations in individual background, recruitment date and recruitment region.

Huambo province. We focus the study on Angola's vast Central Highlands and its center, Huambo province, for three main reasons.[15] First, the Central highlands were at the center of the war. It was in Huambo City, Angola's second largest city, that UNITA proclaimed their own government on the same day MPLA declared independence from Portugal in Luanda, on 11 November 1975. Most parts of the vast Central Highlands were occupied by the two movements at different times and changed hands multiple times, which allows direct comparisons between the members, practices and dynamics of the rival organizations.[16]

Second, this region was *not* at the center of the preceding colonial war from 1961 to 1974. Most anti-colonial activity and guerrilla fighting took place near the coast and international borders, and both movements started most of their systematic mobilization strategies only when the civil war started. At the same time, the rushed exodus of the Portuguese administration, and Angolans employed in it, led to a literal collapse of colonial state institutions in the whole country. This means we can study civil war actors and their policies in an environment, which in the beginning was close to a 'institutional vacuum'.

Third, Huambo province is the most densely populated region in Angola, and ethnically homogeneous (Ovimbundu). Especially in this region, the central strategy

[15]Huambo province is roughly of the size of Switzerland; see Figure 2.2 for a map). The two other provinces of the Central Highlands are the neighboring provinces of Bié and Huila.

[16]In verbal correspondence, the Angolan Institute for the Socio-professional Reintegration of Former Combatants estimated that about 45% of all soldiers demobilized in 2002 Luena program registered in the Central Highlands.

on both sides was to secure the loyalty of *all* individuals living in regions under their control (Roque, 2015). Due to the volatility of territorial control in the region, *"a person's first contact with any political formation was as likely to have been with UNITA as with the MPLA, and the processes that bound people to one or the other movement were similar on both sides."* (Pearce, 2012, 463). Pearce (2009, 4–5) adds that *"political identity was a matter of necessity rather than of conviction. It is for this reason that I use the word 'adherent' rather than 'supporter' when referring to the people who lived under the control of one or other movement during the war, since 'support' suggests a degree of voluntary affiliation which misrepresents the relationship."* In addition, several studies emphasize that, as a result, civilians in the these contested regions would identify as both 'UNITA people' and 'MPLA people' at different stages in their lives depending who ruled their places of residence (e.g. Pearce, 2012). While ethnicity was not at the core of the national conflict, the focus on the Central Highlands thus enables us to study the institutional mechanics of mobilization and recruitment in the near-absence of ethnic factors.

2.4 Research design and data

The empirical analysis is based on your own primary survey data from 759 Angolan war veterans from 34 different localities, collected as part of the Study of Angolan Ex-Combatants (POEMA). The dataset contains detailed and carefully collected retrospective information on military service experiences and pre-service background. The quantitative component of POEMA was supported by an anthropological companion study, which included twelve months of ethnographic fieldwork preceding the survey (Spall, 2015). Qualitative findings have been used to assess the validity of our hypotheses, determine their relevance in the local context, refine the survey questionnaire design, interpret quantitative results and explore underlying mechanisms.

The survey data was collected in partnership with the local Angolan NGO Development Workshop (DW). While NGO activity is generally very limited in Angola, DW has operated in Angola, and especially in Huambo Province, for more than 25 years and was instrumental in making this survey possible.[17] With DW's support, we recruited, screened, trained, and managed a team of enumerators to conduct individual interviews based on electronic close-ended questionnaires.

To ensure as representative a sample as possible, the survey employed three levels of randomization, where the first two involved the primary sampling unit (PSU) and enumeration area (EA) levels. A PSU (*bairro*) was in many rural areas the *comuna* (the lowest tier formal administrative unit), and in urban areas often equivalent to a borough. An EA (*village*) was in rural areas either one settlement or

[17] DW's focus are infrastructure projects and they have done very little work with ex-combatants. This was important to ensure their presence would not have confounding effects on our survey.

multiple very small settlements, and in urban areas equivalent to a neighborhood. In the absence of systematic and reliable veteran population data, we used up-to date data of the total population from the ongoing Angolan census to randomly draw PSUs and EAs, with weights proportional to the population. The sample was stratified by rural and urban locations, according to aggregate census estimates. At each survey site, we engaged with village chiefs, traditional authorities and local administrations to produce listings of all former soldiers residing in the enumeration area (EA). Results were cross-checked to develop credibly complete listings of the local veteran population. Conditional on the reliability of the general population as a proxy for the ex-combatant population, as used in the first stages, the sampling strategy is self-weighting and ensures that the geographic spread across the province is representative. Assuming that we did obtain complete lists of ex-soldiers, the EA-level sample is representative of the EA-level veteran population.[18]

As discussed in Humphreys and Weinstein (2008), three natural first order objections to the validity of this ex-post approach to study recruitment arise: recall bias, survivor bias and other bias from non-representative samples. These concerns will be discussed in detail below.

Table 2.1 reports key descriptive statistics. The statistics show that about 30% of respondents identify as UNITA veterans, and for nearly all of these UNITA was the first (and only) army they joined.[19] 93% were recruited in Huambo province, and 95% were recruited during the civil war (5% joined UNITA or MPLA during the colonial war). The mean respondent was 19.57 years old when he was recruited. 64% of respondents stated that they joined 'against their will'.[20] Figure 2.3 shows the distributions of recruitment date and age. As expected, we observe a lot of variation in recruitment date. The recruitment age distribution is narrowly centered in late teenage years and will be analyzed in more detail below.

2.5 Main results

First, we examine aggregate results from individual-level data on region and date of recruitment and wartime interactions with civilians during military service, to (indirectly) assess the state institutional capacity of UNITA and MPLA and their

[18]See, for instance, Humphreys and Weinstein (2008) for a similar design to study recruitment. The survey design followed standard protocols and was advised by a team of psychologists and – wherever possible – used existing modules that were adapted to the local context.

[19]We probed multiple entries into armed groups and collected data on tenure for each entry, which could include multiple entries into the same or different groups. We coded a veteran's main army as the one he spent most time with. Yet, about 96% of respondents joined an armed group exactly once.

[20]Question: "Did you join against your will?". In the pilot survey, we experimented with more complex questions, but these resulted in low quality data, as respondents were not sure how to interpret these questions. Based on pre-survey results and qualitative insights we asked this arguably simple question, which respondent could answer best. There is no significant difference between the UNITA (68% responding with "yes") and MPLA veterans (63%).

wartime policies.

2.5.1 Institutional capacity and group policies

Territorial control. As explained above, we assume that the group a soldier joined was the one in control of the region where he was recruited. Table 2.2 presents a simple cross-tabulation of aggregate recruitment date and region bins. The region-bins represent the four major regions of Huambo province, while a fifth category ("Outside Huambo") pools soldiers recruited outside the province. The year of entry bins are based on the quartiles of the cumulative distribution function of the date of recruitment. Cell entries denote the share of UNITA recruits of all veterans that were recruited in a given region at a given time and reveal that there was substantial variation in the UNITA share over time and region. Simple F-tests confirm that the UNITA share over time in a given region as well as the share across regions at a given time are not constant: 8 out of 10 row- and column-based tests reject equality at the 90% confidence level.

Figure 2.4 plots the date distribution of all UNITA (red) and MPLA (blue) recruits in a given region. For each region, the graphs reveal considerable variation over time in how likely it was (ex-post) to join a certain army. There is no region where individuals were consistently more likely to have joined one army than the other over the course of the war. Even though both armies often claimed control over pockets of territory in the same region at the same time, the negative correlation in the distribution over time between the two armies is striking (conditional on region). At a time when the number of recruits into one army increased, we often observe a simultaneous decrease for the other army. While these are broad regions, the patterns contribute further evidence for a central role of territorial control for recruitment and suggest that variation in aggregate recruitment numbers over time is closely related to shifts in territorial control in a particular region.

Public good provision and extraction. Next, we look at soldiers' self-reported involvement in group policies of extraction and provision of public goods and services that they helped to carry out. Table 2.3 compares the raw shares of soldiers ever involved in a certain activity by UNITA and MPLA. The top panel suggests that many soldiers of either army report involvement in extractive activities, including recruitment of new soldiers. While the differences between armies are modest, the levels are generally slightly higher for UNITA soldiers. The largest difference is in food extraction, which occurred very frequently in the case of UNITA. We sought to distinguish involvement in two different categories of recruitment activities, namely recruitment (primarily) 'by consent' and (primarily) 'by force', based on the subjective assessment of the respondent. Across armies, 67% or more report having been involved in each form of recruitment, which shows that recruitment was institutionalized in either army. Going beyond binary reports of any

involvement in the implementation of extractive policies, Figure 2.5 displays results at the intensive margin. The distributions across the "very often", "often", "sometimes", and "rarely" bins reveal that also in terms of frequency distributions of the groups were fairly similar.

The results reported in the bottom panel Table 2.3 confirm that local populations also received considerable amounts of public goods and services from the armed groups. Now levels are significantly higher among MPLA soldiers, even though the differences are again modest in magnitude. For both armies, more than 80% of respondents report having been involved in protecting civilians under their control, while the least frequent activity of public good provision appears to have been the supply of arms.

While only suggestive, these findings support historical evidence that both groups engaged heavily with civilians. In our framework, this suggests that both groups had a certain degree of institutional capacity to organize and implement these policies systematically.

2.5.2 Selection into UNITA: background

In the second part of the empirical analysis, we now use individual pre-service background profiles and information on region and date of military entry to analyze recruitment strategies employed by UNITA. The basic idea is to assume that MPLA used conscription (in practice) and we then analyze whether and how UNITA recruits were systematically *different* from MPLA recruits, in terms of background or recruitment circumstance.

We test how UNITA recruits differed in terms of family background characteristics compared to MPLA recruits in four ways: (i) comparison of the unconditional means of pre-treatment variables (bivariate analysis); (ii) comparison of conditional means (via a linear probability model); (iii) comparisons based on averaging over all possible specifications of included variables (using a Bayesian model averaging approach); and (iv) comparison of the distribution of the probability of joining UNITA (via a logit model). The extensive list of background covariates was constructed based on previous theoretical and empirical findings on political, economic and social incentives to join a rebel group. The list of background variables is similar to those utilized in the two most robust quantitative studies of rebel recruitment in Humphreys and Weinstein (2008) and Blattman and Annan (2010), and was adapted to the Angolan context.

Table 2.4 presents unconditional differences in background variables. As expected, there is essentially no variation in terms of ethnicity as basically all respondents are ethnic Ovimbundu and spoke Umbundu at home. Yet, the other variables reveal striking patterns. On the one hand, there is considerable variation in many background variables within either army, i.e. not 'everybody had the same background'. For instance, about one quarter of respondents in the full sample were

from the IECA church, which was sometimes associated with UNITA leader Jonas Savimbi (Spall, 2015), in about half of the cases the father had some formal schooling, and about 15% report that household members had lost their lives due to war violence before the they entered the military. While standard deviations are omitted for reasons of space, there is considerable variation in both groups (available upon request). Importantly, however, this variation does not result in systematic difference across armies as indicated by the p-values reported in the last column. The only exception is an urban location dummy indicating whether the soldier was recruited in an urban area, which is considerably higher for MPLA. This result is not surprising, given that MPLA held urban areas more often than UNITA.

In terms of family background, these results mean that none of the considered characteristics is a robust predictor of having joining UNITA versus MPLA. The analysis below tests the robustness of this finding in more complex models, which will use the set of variables labeled "core variables" in Table 2.4. We drop other variables as there is either no variation, as in ethnicity, or they are strongly correlated with one of the core variables.

Robustness tests. Regressions of a binary army indicator variable on the full set of family background characteristics finds that also in conditional models no family background variable is a robust predictor of UNITA recruitment (Table 2.5).[21] Most coefficients are very small in magnitude and none is statistically significant. The models in columns 1 to 5 include region-, municipality-, year of entry-, and quintile of year of entry-fixed effects, while column 6 includes an urban location indicator and column 7 no fixed effects at all. All specifications use classical standard errors to be as non-conservative as possible. More conservative standard errors, such as clustered by entry region or year, lead to smaller t-statistics on the coefficients (not shown, but available upon request).

As the urban dummy may be endogenously determined, which will be discussed below, and is highly correlated with the municipality dummy indicators, it is excluded from most specifications. Yet the results hold conditional on urban location (column 6). It is worth noting that the simultaneous inclusion of year of entry and municipality fixed effects has considerable explanatory power (column 1), while in the absence of any recruitment location and date dummies, the adjusted R-squared is even negative (column 7). The strong role of the *combination* of time and place for the armed group outcome is consistent with qualitative analyses of the civil war (e.g. Pearce, 2012).

Table 2.6 shows results from repeating the least conservative regression (without fixed effects) for different dates of entry separately. As above, the recruitment date-bins denote quartiles of the full distribution of dates. A few variables, including number of livestock or total number of previous household recruits are significantly correlated with UNITA recruitment in certain bins. Yet, none of these effects is systematic across bins, and, in fact, the sign of all of these coefficient changes across

[21] The binary army indicator ('UNITA') equals one for UNITA veterans and zero for MPLA veterans.

bins.

Next, we test the sensitivity of our results to model specifications by using linear Bayesian Model Averaging (BMA) analysis. It might be the case that the coefficients are zero in bivariate and the fully specified models, but not in others. BMA analysis accounts for uncertainty over the 'true' theory of influence of these factors by computing posterior probabilities over the model space spanned by the vector of covariates. That is, the algorithm considers models with all possible combinations of the possibly relevant covariates, and constructs a weighted average over the most likely models. This exercise extends the analysis presented in in two ways. First, BMA offers a fully transparent model selection procedure, which guards against cherry picking models in which one's preferred coefficient is significant or, as in the analysis above, insignificant. Second, by calculating posterior distributions of coefficients and models, BMA analysis also enables to assess whether a given variable consistently contributes to models' explanatory power (Montgomery and Nyhan, 2010). If, for instance, family wealth positively co-varies with UNITA recruitment, but models that include family wealth have a very low cumulative posterior probability of being correct, the averaging procedure will assign a low weight to that variable.

The Bayesian estimator in this analysis uses a multivariate Gaussian prior on the parameters. The unconditional BMA estimates are then obtained as a weighted average of the estimates from each possible model in the full model space. Weights are proportional to the marginal likelihood for the model, i.e. models that are 'more likely' are assigned 'more weight'. Formally, the quantities of interest are hence the model-weighted posterior distributions for the covariate coefficients.

The evidence presented graphically in Figure 2.6a confirms that none of the 13 background covariates is a robust predictor of whether a soldier joined UNITA or MPLA. Based on the 8192 (=2^13) model specifications considered by the BMA algorithm, the means of the posterior distributions are extremely close to zero, with *t*-ratios in absolute value of .27 or less. A covariate is considered to be robustly correlated with the outcome if the *t*-ratio exceeds 1 in absolute value. In terms of posterior inclusion probabilities, all inclusion probabilities are less than .1, while the standard threshold for robust predictors is .5. Adding an urban location dummy indicator Figure 2.6b, does not change the means of the posterior distributions noticeably for the core set of covariates. For the urban coefficient BMA estimates the mean of the posterior distribution at -.211 with a *t*-ratio of 6.52, or equivalently a posterior inclusion probability of 1.00. These estimates again confirm the results from the baseline analysis.

As a final robustness exercise, we look at the distribution of the predicted probability of UNITA recruitment based on non-linear models. The probabilities are predicted from a logit regression of the army indicator on the full set of core covariates. As shown in Figure 2.7, the distribution of UNITA recruitment probability is extremely narrow for both sub-samples. The two distributions are very similar and

overlap substantially, with no support above .41, which adds further evidence that none of our covariates is a reliable correlate of UNITA recruitment.

2.5.3 Selection into UNITA: location x time

So, what determines the variation in the army outcome? The regression models in Table 2.5 revealed that the simultaneous inclusion of time and location fixed effects has considerable explanatory power (column 1). This is consistent with the patterns in Figure 2.4, which revealed a lot of temporal variation in MPLA and UNITA recruitment within regions.

Figure 2.8 presents further results on the role of the interaction of recruitment region and date for the army outcome. Figure 2.8a presents the distribution of probabilities predicted from a logit regression of UNITA recruitment on quintiles of the date-of-entry distribution (reference category: 'earliest' quintile) and on recruitment region (five regions; reference category: 'North'). As expected, the distributions are a lot more dispersed now, but there is still substantial overlap between armies. Figure 2.8b presents related results after including the full set of core background covariates. The plot shows that compared to the original plot above this leaves the distributions virtually unchanged. These findings provide further support for important role of the interaction of date and region and against individual background in determining the army outcome.

Migration. Above, we also found that urban and peri-urban recruitment locations are (overall) correlated with MPLA recruitment. This means that there might be a selective process into MPLA based on migration into urban areas, which may be both forced and voluntary migration. If recruits from urban areas or recruits that had moved to urban areas are a selective group in terms of background, the significant role of urban and peri-urban locations may then mask systematic underlying differences. To investigate the role of migration, we use the binary indicator of whether a veteran's household had been displaced as a consequence of the war before he joined the military, which likely does not pick up all but at least some aspects of migration processes.

Column 1 in Table 2.7 investigates the background correlates of having been recruited in an urban area, without conditioning for the displacement indicator (which is likely determined endogenously).[22] We find that only radio possession is significantly correlated with being recruited in an urban area, which is likely related to the availability and usefulness of radios in urban areas (column 1). Including the displacement indicator reveals that, as expected, displacement is correlated with

[22]The dependent variable is the binary 'Urban' indicator, which equals one for recruitment in an urban or peri-urban area and zero for recruitment in a rural area. Estimates are from simple OLS regressions.

urban recruitment, but all other coefficients remain basically unchanged (column 2). To investigate the role of migration further, we run the army outcome regression separately for the displaced and non-displaced sub-samples, defined by the displacement indicator. We observe in columns 3 to 6 that for both sub-samples the urban indicator is a negatively associated with recruitment into UNITA, but we do not observe differences based on background variables in any model. Specifically, this holds true when we control for urban location (columns 4 and 6). We conclude that we do not find any robust evidence for a migration-based selection into MPLA (via urban areas).

2.6 Nature of recruitment

In the final exercise of the empirical analysis, we now try to assess whether recruitment was consistent with conscription.

2.6.1 Recruitment age

Figure 2.9a plots the statistical distribution of recruitment age across time, in the form of quintiles of the date of entry-distribution. While the age at military entry varied greatly for the earliest recruitment years (shown in red), the distribution stabilizes rapidly, with a normal and symmetric age distribution that is clearly centered in late teenage years. The earliest quintile is composed by individuals recruited during the colonial war, where mobilization was different. Thus, the difference in this group is not surprising, and suggests that recruitment mechanisms changed with the start of the civil war.

Looking at the recruitment age distribution of MPLA and UNITA separately, Figure 2.9b and Figure 2.9c demonstrate that this stabilization of the age distribution with the start of the civil war holds for both factions. The distributions for the later quintiles are very similar, where the only difference is that the mean of the UNITA distribution is slightly shifted toward younger ages. The overall mean recruitment age for MPLA soldiers was 20.05 [95% CI: 19.57,20.52], while that for UNITA soldiers was UNITA 18.45 [95% CI: 17.46,19.43]

These results are consistent with standard patterns of conscription, which is usually age-centric, i.e. the main conditioning factor for the date of entry is age (among the eligible sub-group, which are usually able-bodied men). The link between date of entry and date of birth is shown directly in the locally weighted regressions presented in Figure 2.10. Figure 2.10a illustrates the close relationship between date of entry and date of birth, while Figure 2.10b presents the corresponding relationship between date of entry and age at entry.

A key question is whether we can distinguish the coercive recruitment advanced in this paper from abduction, which often targets relatively young people. To address this question, we compare the age profiles of MPLA and UNITA recruits to that of abducted soldiers from Northern Uganda.[23] Figure 2.11 plots the age distributions and demonstrates that most of the probability mass among Ugandan soldiers is at a significantly younger age than that of UNITA and MPLA soldiers recruited during the civil war period.[24] While the UNITA mean is slightly below MPLA's, these patterns provide evidence against abduction, at least in the form of child soldiering, as UNITA's dominant strategy.

2.6.2 Internal cohesion

Next, we investigate differences in exposure to wartime sexual violence across armies as an indirect test of abduction. If Cohen (2013) is correct, we would expect to observe systematic differences when comparing an abducted to a conscripted army. Table 2.8 displays the results from simple linear regressions and finds no substantive differences between armies.[25] UNITA veterans are not more likely to report having experienced situations of sexual abuse of civilian women during the war. Alternatively, seeing no differences in sexual abuse could also mean that we are comparing two cases of abduction. In such a scenario we would expect to see high level of sexual abuse on both sides. Yet, as also reported Table 2.8, the overall mean exposure to sexual violence of .2 is comparably low. This is consistent with the findings of Chapter 4, which suggest that both groups used sexual violence only moderately and selectively.

2.6.3 Material incentives

Finally, we analyze regular compensation as an indirect test for professional armies of volunteers. If UNITA consistently used some of its revenues from the diamond trade, or extracted great amounts of tax money, to hire and remunerate soldiers, a professional army would be a viable option. We therefore examine now whether UNITA soldiers were more likely to receive material rewards on a more or less regular basis. Table 2.9 finds strong and systematic differences in the *opposite* direction: UNITA soldiers were significantly *less* likely to receive compensation on a regular basis, both in monetary and non-monetary terms. Especially the difference in monetary rewards is very large and only 6% of UNITA veterans report having received monetary compensation on a regular basis. The differences are very

[23]The auxiliary data from Northern Uganda was collected in a study by Blattman and Annan (2010) and is public.

[24]We omit those recruited during the colonial war here and focus on date of entry quintiles two to five.

[25]The binary 'Wartime sexual violence' indicator equals one if the veteran reports having been exposed to at least one situation of sexual abuse by his group against civilian women during the war, and zero otherwise.

meaningful, and robust to the inclusion of region and time of recruitment dummies, pre-service background characteristics, tenure, and rank. Rank may obviously be endogenously determined, but the difference in material rewards could reflect an asymmetry in rank across armies. Yet, even conditional on rank, the difference does not change noticeably and stays very large. The fact that UNITA offered so little in terms of regular compensation basically rules out an interpretation of a professional army and provides more evidence against traditional theories of rebel recruitment based on opportunity cost-arguments.

Taken together, these comparative analyses point away from abduction or hiring volunteers as dominant forms of recruitment by UNITA. The analysis does *not* rule out that these other forms were important, yet the patterns observed suggest that conscription played a dominant role.

2.7 Discussion

This section discusses the robustness of the empirical finding that UNITA was able to and did use conscription as a dominant form of recruitment, by assessing biases that could drive the results and qualitative evidence.

2.7.1 Robustness

In terms of data, three natural first-order threats to the validity of our analysis are recall bias, survivor bias and other forms of bias from non-representative samples (Humphreys and Weinstein, 2008).

Recall bias poses a threat to any study using retrospective data, and one key source of error is misreporting due to incorrect memory and lack of knowledge. A main concern is that respondents may misreport because they need to recall information and situations from a long time ago, or because they did not perceive these as important. Psychologists have emphasized and developed data collection techniques based on the premise that individuals may recall information better when they can relate them temporally to incisive events (e.g Freedman et al., 1988). Being enlisted into the military is a defining life event that should hence increase a respondent's ability to recall outcomes just before this happened reasonably accurately. Similarly, situations and events during military service are plausibly incisive experiences themselves and therefore less prone to memory failure. Two observations from fieldwork strengthen the assumption that the recalled service and pre-service information is reliable. First, "I don't know" or "I can't remember" responses were very rare. Second, the complementary ethnographic research by Spall (2015) gathered vast and detailed information on 'life before the military' and 'life in the military', which all interview partners were able to provide.

A second important concern is that respondents may misreport their year of birth, which is a key variable in the study. To address this issue, we asked for information on date of birth and current age separately, in separate interviews. In an initial household interview with the veteran and his partner (if in a partnership) information on the ages of all household members was collected, including the veteran's. In the individual interview with the veteran, we asked for his date of birth. If necessary, enumerators provided help to determine the year of birth relying on an extensive list of important events in Angolan history. This entry was then immediately compared to the age entry (from the first interview) by the enumerator. For inconsistent answers, enumerators revisited the questions and provided assistance in determining as correct answers as possible.

A final issues is that individuals may misreport for personal reasons, which is especially relevant for sensitive and traumatic experiences. A well-documented behavior is the "embroidery" of personal experiences in such cases (e.g Mausner and Kramer, 1985). Yet, in addition to their ability to provide the relevant information, veterans also exhibited an overwhelming openness toward discussing their service and pre-service lives in both qualitative and quantitative interviews. In the quantitative component, no respondent opted to not answer questions regarding pre-military life, skip questions or quit the interview. As expected, a few respondents did opt to skip specific questions on military life, but the number of such cases was low and not systematic, i.e. not concentrated in specific questions or respondent characteristics. For these reasons, we trust that the vast majority of the questions were not particularly sensitive (in this context) or difficult to answer, and that our data are not affected by systematic misreporting.

Statistically speaking, misreporting introduces measurement error, which typically adds noise to the signal and leads to simple losses in statistical power. The fact that we find a series of non-significant predictors of army selection could suggest such an issue. However, looking at statistical properties of key coefficients, the estimated standard errors are small and the effects rather precisely estimated. For to actually introduce significant bias, the error would have to be systematic across rebel and government soldiers. Given the balance across pre-service characteristics, it seems unlikely that such a systematic difference exists. Taken together, we argue that recall bias is not likely to confound results.

Another concern is survivor bias. As explained by Humphreys and Weinstein (2008), survivor bias in this setting can be interpreted as a form of sampling bias: a natural limitation of conflict-related surveys is that potential respondents who died can obviously not be sampled. This concern emerges if non-survivors' distributions of pre-service or service outcomes, or a characteristic correlated with any of these, are systematically different from those of survivors. A simple example is that soldiers from poorer families were more likely to die in combat. Yet, a substantial effect on our results would again require an asymmetry across the two armed groups. For instance, that one group specifically targeted poorer people more than the other group. While plausible, the empirical findings point away from this concern. Similar

to the argument above, we interpret the remarkable balance across pre-service covariates, the absence of any strong conditional selection based on these, and the stability and symmetry of the age distribution as strong arguments against such an asymmetry and mechanism.

Perhaps the most serious concern in practice are other forms of non-representativeness of the sample. In the absence of official local veteran population lists, the degree of representativeness hinges on the quality of the local sampling frames we built with local authorities. These frames may be incomplete and not representative. A central concern is that local authorities may possibly favor those who served in the same faction they served themselves or favor MPLA veterans, as many village chiefs, for instance, receive regular, small compensation from the formal MPLA authorities. During the survey we made a great effort to work with *all* relevant local actors and authorities we could identify, to provide enough time for them to produce reliable population lists and then cross-validate lists from different sources. Wherever applicable, these sources explicitly included civil society groups affiliated or associated with UNITA and local branches of the MPLA and UNITA political parties.

Two related insights from pre-survey fieldwork are worth noting in this regard. In the initial survey design, we intended to sample a non-combatant control group and (pre-) stratify by non-combatants, UNITA and MPLA combatants, based on three separate local sampling frames for these groups. Before the pilot phase, the qualitative pre-survey analysis had already produced findings in line with historical accounts that military service was universal (Spall, 2015). The pilot phase added additional evidence by revealed that in most locations the non-combatant group of adults was either very small and very special, i.e. people born with disabilities, or even non-existent. In some rural areas even the *soba*, the village chief and highest local authority, had served in the military.

Not surprisingly, local sources (and veterans themselves) would consistently have accurate knowledge who was with an armed group during the war to produce reliable combatant lists. The second insight was, however, that local sources (and veterans themselves) would consistently fail to produce separate lists for UNITA and MPLA veterans. More broadly, individuals across Huambo province use "tropa" (literally "troops") to categorize individuals with respect to military service. This distinguishes someone who 'was with the troops' from all others, while the specific side is often, perhaps surprisingly, unknown. After careful review of piloting results, we concluded that the meaningful and valid empirical comparison is UNITA veterans versus MPLA veterans, and to produce and sample from reliable lists of the local population of all combatants. Based on these insights and the arguments regarding asymmetry presented above for non-survivors, it seems difficult to imagine why and how the group left out may be different in a matter would affect the results.

2.7.2 Qualitative and anecdotal evidence

Our empirical findings are in keeping with the historiographic and ethnographic literature on wartime politics and political identity described above (e.g. Pearce, 2012). These studies suggested no strong 'selection' into either side based on conventional, individual factors associated with rebel recruitment and mobilization into armed groups.

On MPLA recruitment specifically, interviews with government officials and veterans suggest that, at least in theory, state conscription was consistently enforced, and universal for young men throughout the course of war (Parsons, 2004*b*, 2006; Spall, 2015). As noted above, only scattered and anecdotal evidence exists on UNITA recruitment, which we will now discuss briefly.

While not focused on recruitment, Porto, Alden and Parsons (2007) provide the most detailed existing analysis of UNITA soldiers. Their study investigates UNITA soldiers' demobilization and reintegration experiences in the Central Highlands, based on a mixture of qualitative and quantitative techniques. While they quote a former UNITA General to suggest recruitment was often "on the basis of some political project" (Porto, Alden and Parsons, 2007, 71), the study also notes that the issue of "possible abduction and forced recruitment [...] will require considerable attention and future research – adequate treatment of it is naturally outside the specific objectives of this project" (Porto, Alden and Parsons, 2007, 48). This summarizes the general theme that emerges from qualitative description of UNITA recruitment: it was coercive in some sense, but there is uncertainty regarding the question what the general dominant nature and patterns were, and when. In a related analysis, Parsons (2006) describes how UNITA in newly conquered villages would often invest substantially in relationships with the traditional local authorities for their help with identifying young men for compulsory military service, which strengthens our argument that recruitment was institutionalized by UNITA. Parsons (2006) also confirms anecdotes that in volatile areas members of the same family would often end up fighting on different sides, which corroborates our interpretation of the crucial importance of territorial control. Parsons (2004*b*, 53) suggests that UNITA tactics varied and this included times when it "resorted increasingly to the use of coercion and intimidation."

Some of these academic perspectives are also consistent with wartime reports on recruitment by humanitarian actors and news agencies. As a representative example, the Integrated Regional Information Networks agency (IRIN) reported that *"in the last few weeks, [MPLA] radio announcements have been instructing young men aged between 18 and 20 to report to military bases around the country to register for military service, as is normal at this time of year"* (IRIN, 2001, 1), and adds that *"[witnesses] tell how as soon as [MPLA] recapture a village from UNITA, the men are seen as fair game for recruitment [...] UNITA are also known to forcibly conscript, and have also seized children for military service."*(IRIN, 2001, 2).

2.7.3 Multiple strategies

Two conclusions are apparent. First, the qualitative accounts emphasize further that there were probably multiple strategies of recruitment by UNITA. It is important to note in this regard, that even in strong states recruitment usually includes multiple strategies. Conscript armies, for instance, will normally *always* also include volunteers (Mulligan and Shleifer, 2005). Second, UNITA's recruitment portfolio certainly included methods relying on the use of physical force. As noted above, in many states effective use of conscription is not entirely free of the use of force. Noncompliance with compulsory military service requires an enforcement policy, which in strong states will be based on the rule of law. Yet, in states with weak legal capacity, enforcement of compliance will often imply the use of physical force. Hence it would not be surprising but rather expected that there was no single policy of recruitment and violent enforcement (on either side).

So, the relevant empirical question is whether conscription was the *dominant* strategy, rather than abduction or press-ganging. Two final reflections explain further why there is reason to believe that this was the case.

First, abduction and press-ganging may be efficient short-term strategies, but it seems implausible, from an economic point of view, to be able to wage a high-capacity military competition for 27 years based on these forms of recruitment. Organizing abduction and sustaining an abducted army, for instance because of a lack of cohesion, is very costly, and likely can't be done in a way that would deliver the military successes UNITA achieved. Second, the Angola literature and this article emphasize UNITA's state-building mission and large-scale investments in legitimacy and social support. Of course, there is abundant documentation of UNITA's use of extreme and widespread violence against civilians, especially towards the very end of the war. Yet it appears that much of this violence occurred in select and temporally bounded periods (see also Chapter 4 of this thesis). Together with the organizational objective and narrative UNITA presented to the people, this suggests that also from a political view it was unlikely that UNITA's primary strategy of mobilization was abduction or press-ganging. Based on these arguments, it seems more plausible that conscription was the preferred strategy, and that stronger forms of coercion were used, for instance, when the organization lacked the institutional capacity to organize conscription (administratively), enforce it non-violently, or had some specific reason to use recruitment based on physical force.

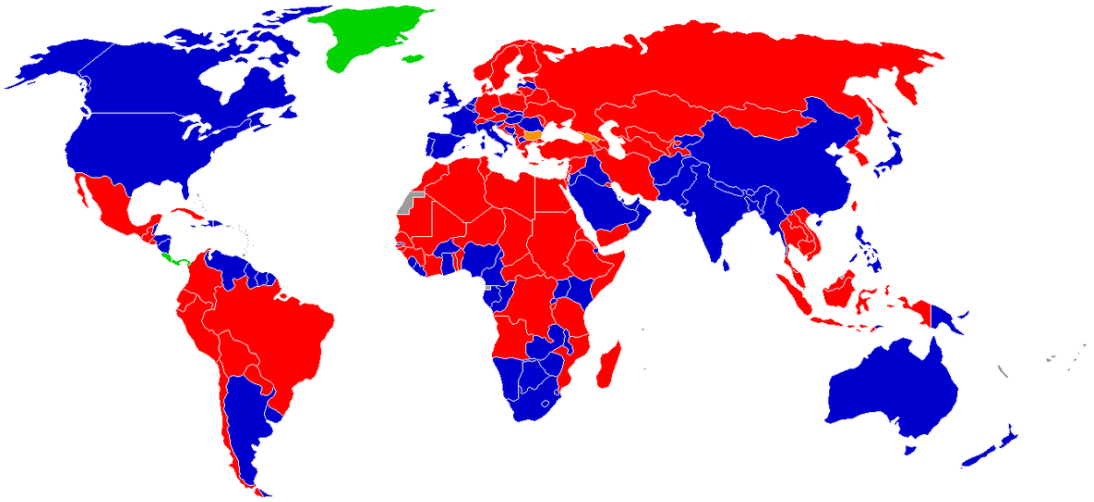
2.8 Conclusion

This paper develops and tests a simple theoretical argument on rebel recruitment motivated by the literatures on rebel governance and state capacity. The paper first argues that non-state actors may have the institutional capacity to rely on 'state-like'

recruitment in an effective and sustainable way, which primarily relies on tax compliance rather than individual motivations or the use of physical violence. Following state logic, the paper then predicts that an actor that *has* the ability to tax, and seeks to build a large army or engages in large-scale belligerent activity, will choose conscription.

The empirical results support the theoretical predictions for the case of the UNITA rebels in the Angolan Civil War. Taking all evidence into account, the paper concludes that there is strong evidence that UNITA was a) able to use, and b) did use tax-based recruitment in the form of conscription in practice. Other forms of recruitment that involved the use of physical force likely also occurred, which is not surprising, but rather expected, as both state and non-state recruitment in most cases relies on multiple strategies for recruitment. Which strategy UNITA preferred, and when, is ultimately difficult to test, but the evidence suggests that conscription was the dominant strategy in practice. An interpretation consistent with the theoretical prediction on the preference for conscription is that stronger forms of coercion and physical force were used in times when the organization temporarily lacked the institutional capacity to organize and/or non-violently enforce conscription, or deliberately chose to use abduction for some specific war-related or other reason. These findings challenge conventional theories of rebel recruitment and emphasize the role of institutional capacity for rebel tactics, which previous studies have not studied in great detail.

Figure 2.1: World map of state conscription



Note: Red shading denotes 'Conscription', blue shading denotes 'No conscription', green shading denotes 'No armed forces'. Source: Wikipedia (2016).

Figure 2.2: Map of Huambo province and regions

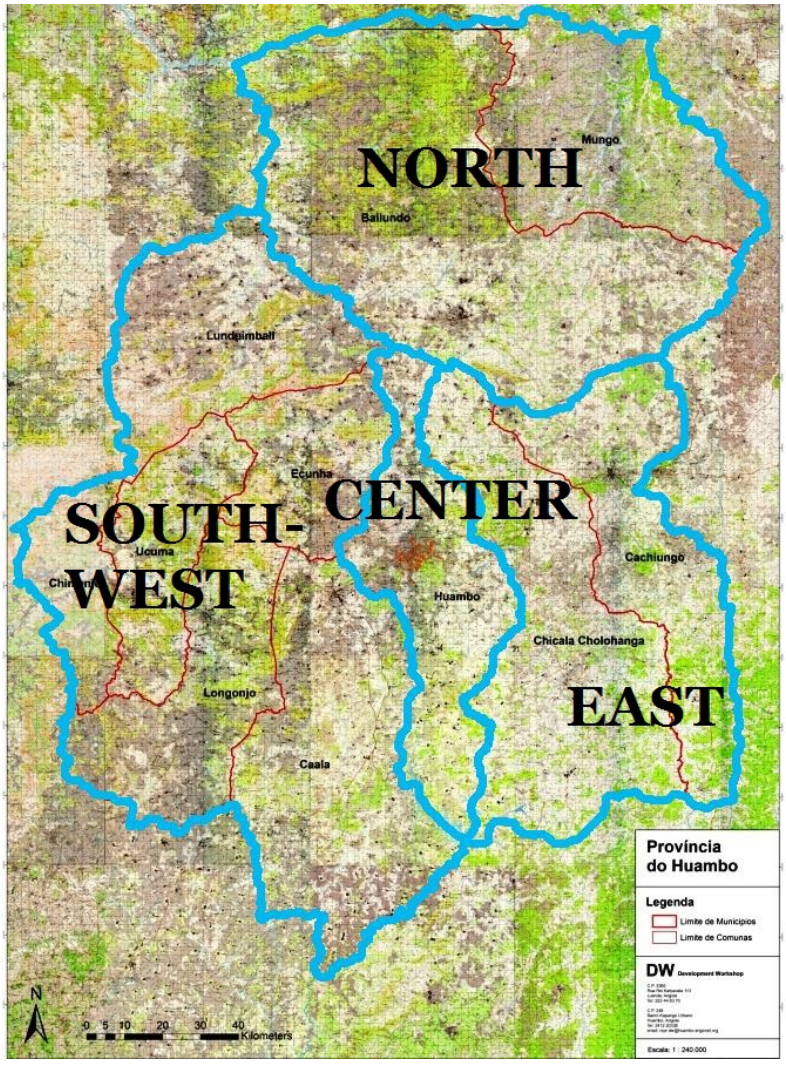
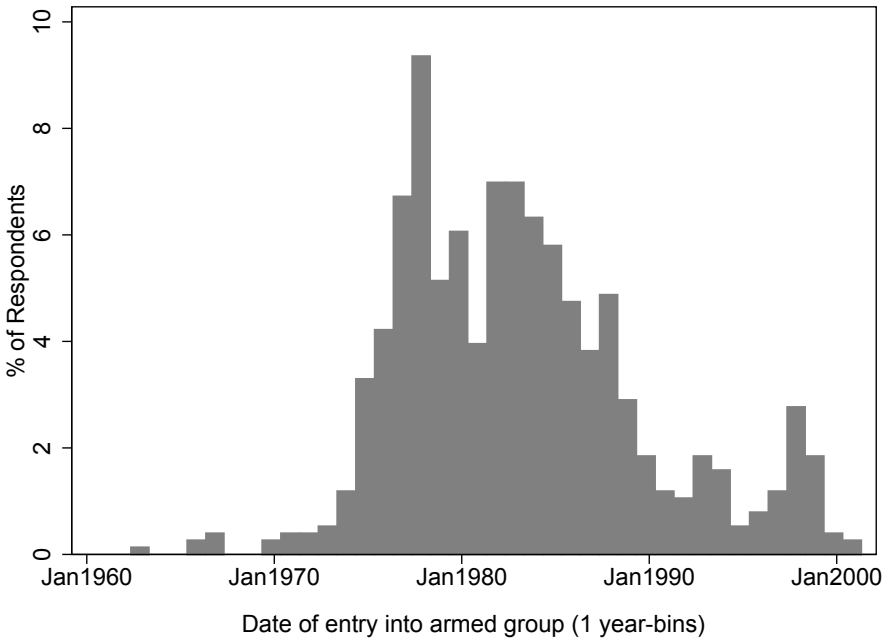


Figure 2.3: Distributions of recruitment date and age

(a) Recruitment date



(b) Recruitment age

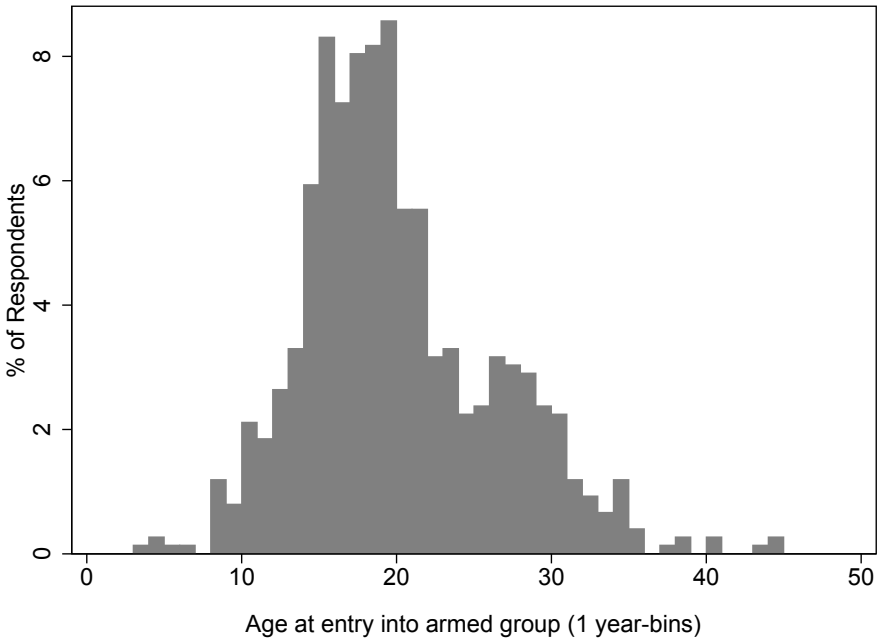


Figure 2.4: Distribution of recruitment date by recruitment region

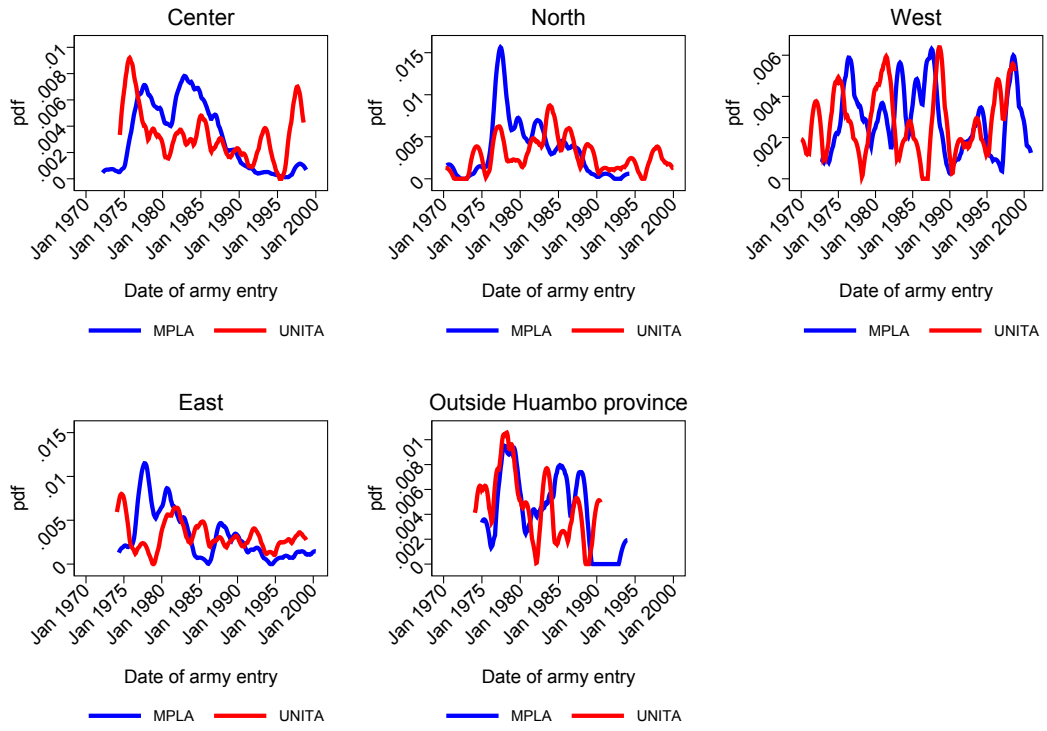
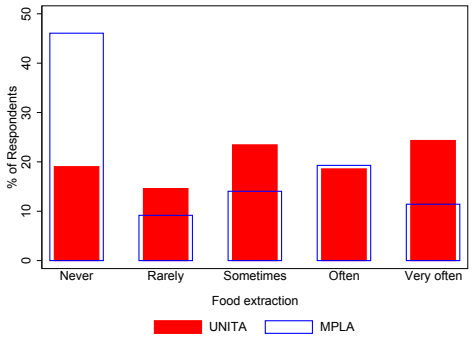
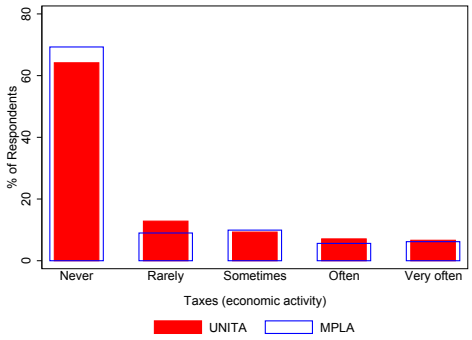


Figure 2.5: Institutional capacity

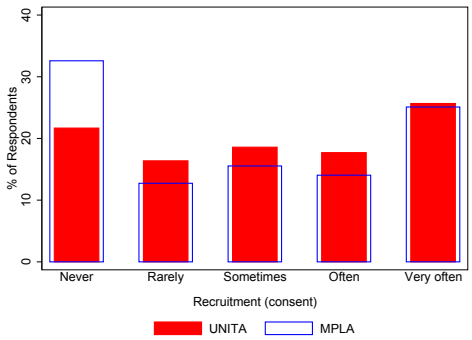
(a) Food extraction



(b) Taxes (economic activity)



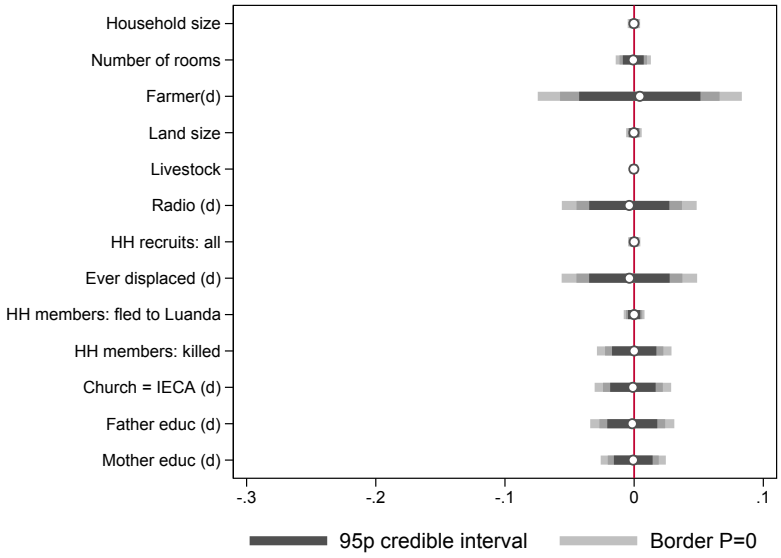
(c) Recruitment (consent)



(d) Recruitment (force)

Figure 2.6: Posterior probability distribution from Bayesian Model Averaging

(a) Background variables



(b) Background variables and urban/peri-urban indicator

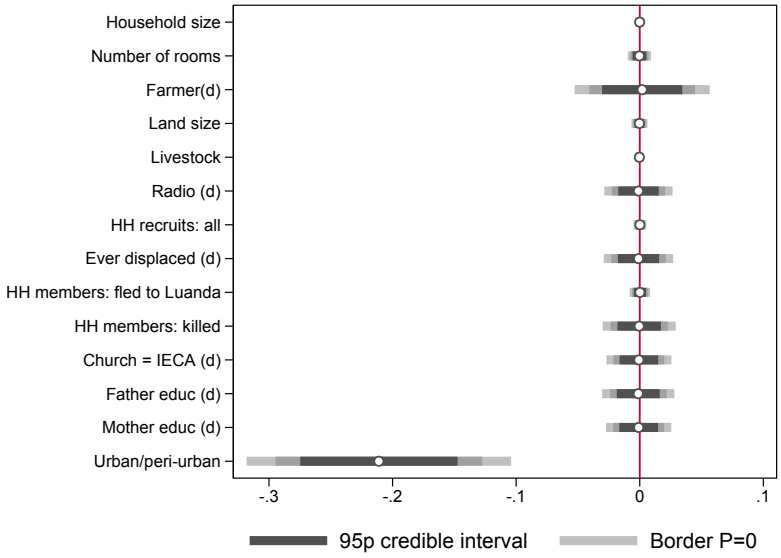


Figure 2.7: Predicted probabilities of joining UNITA from logit model 1

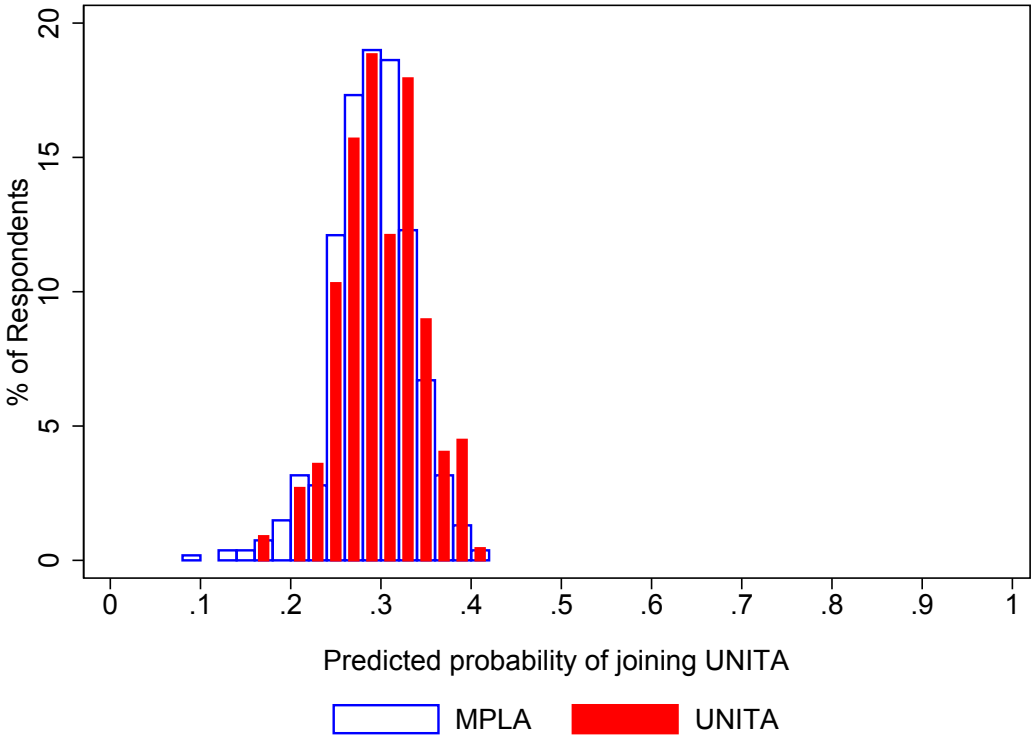
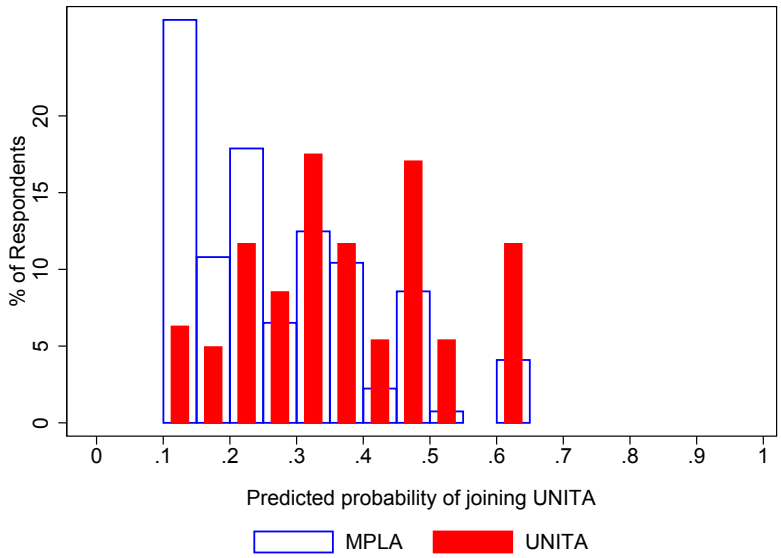


Figure 2.8: Predicted probabilities of joining UNITA from logit model 2

(a) Based on recruitment date and region



(b) Based on recruitment date and region and background variables

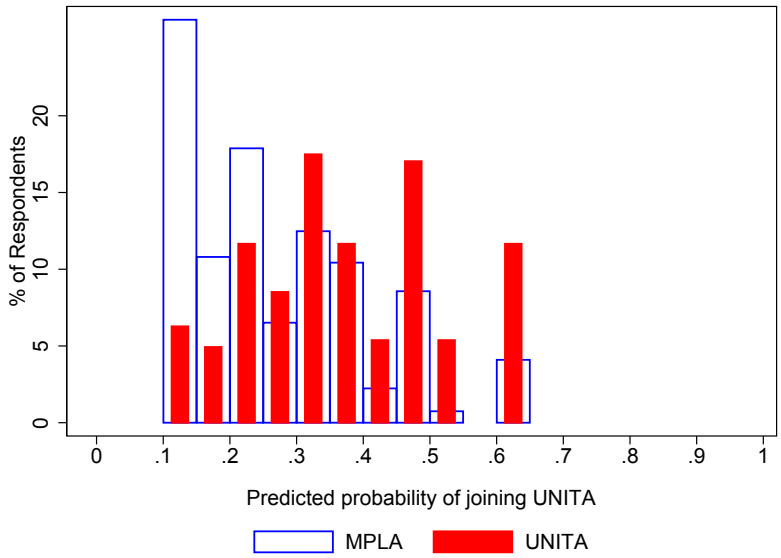
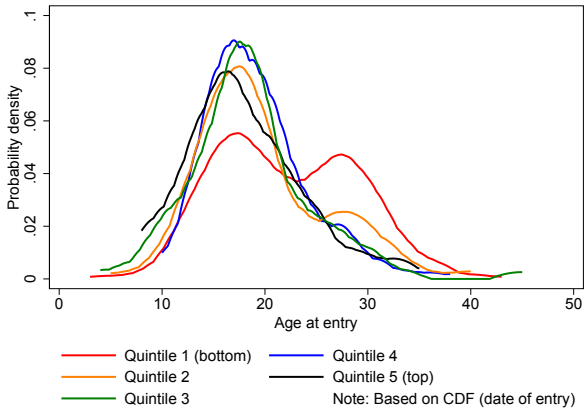
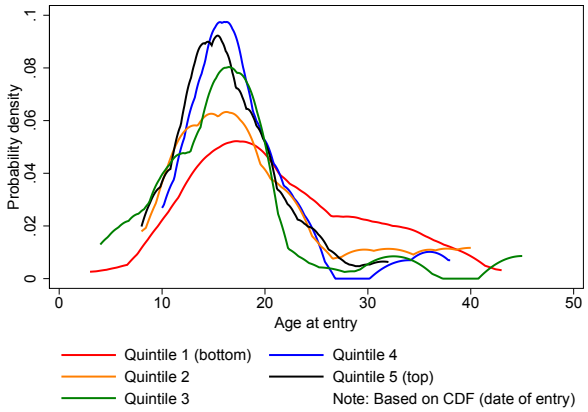


Figure 2.9: Distribution of age at entry over time

(a) Full sample



(b) UNITA



(c) MPLA

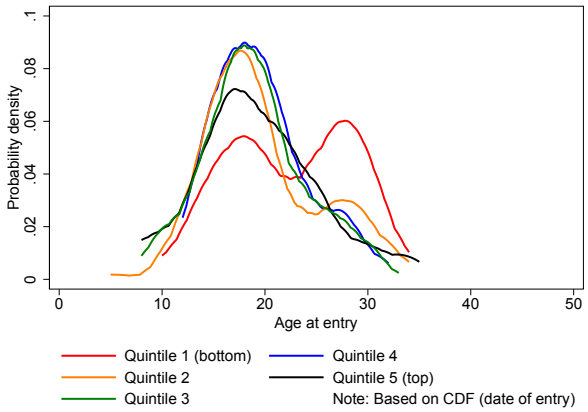
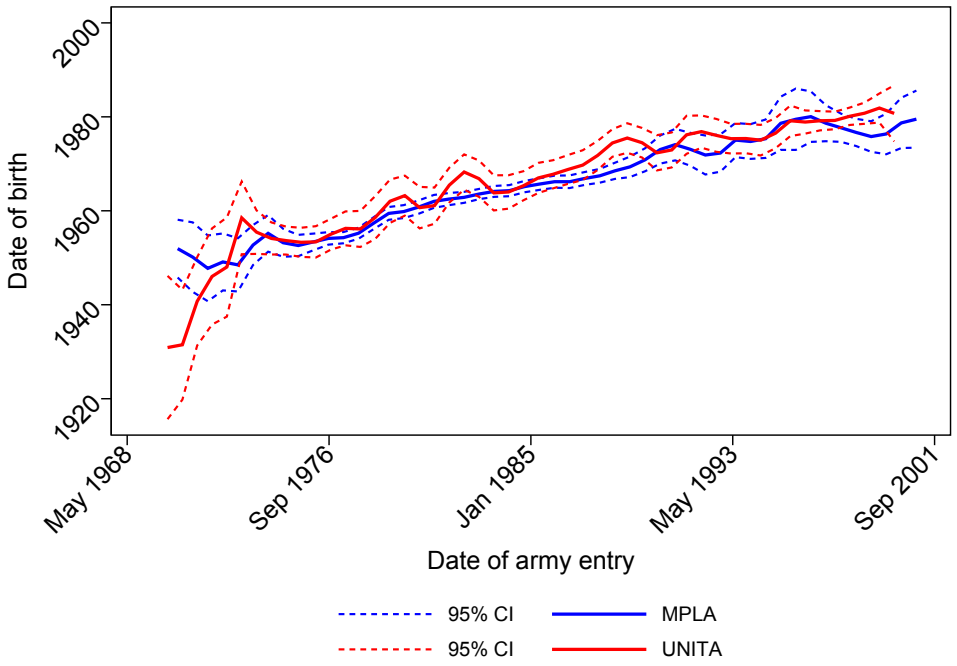


Figure 2.10: Year of birth and age at entry over time

(a) Date of birth



(b) Age at entry

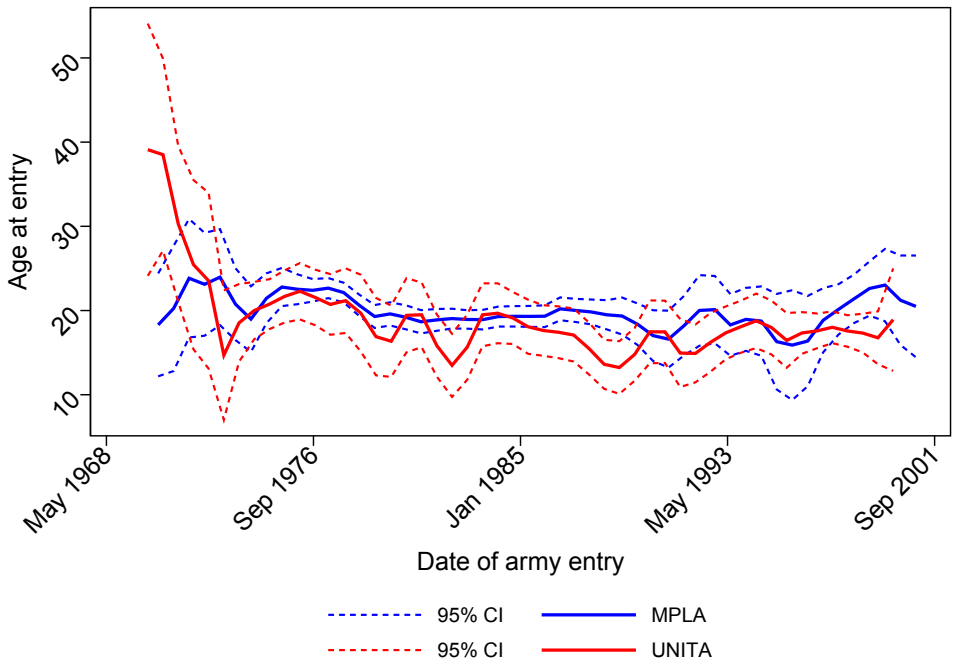
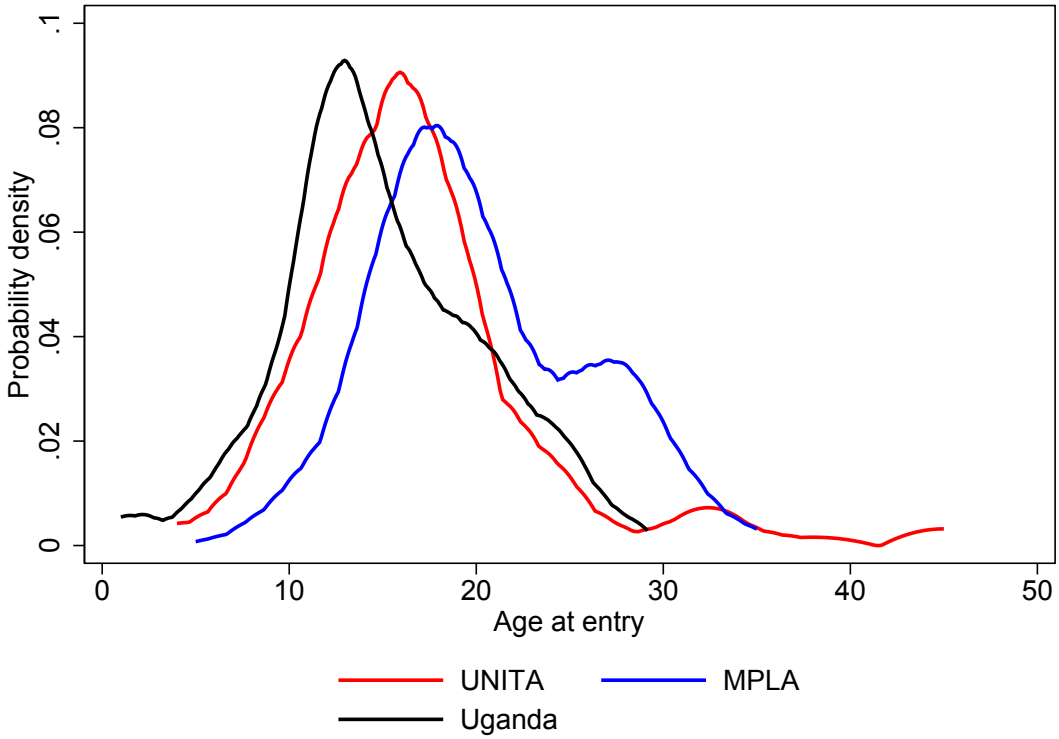


Figure 2.11: Distribution of age at entry across groups



Note: Auxiliary data included from abducted child soldiers in Northern Uganda (Blattman and Annan, 2010).

Table 2.1: Key descriptive statistics

	Mean	SD	MIN	MAX
UNITA (main army, d)	0.30	0.457	0	1
UNITA (first army, d)	0.31	0.464	0	1
Age at (first) entry	19.57	6.270	3	45
Date of (first) entry	1982.84	6.583	196	2000
Recruited in Huambo province (d)	0.93	0.248	0	1
Recruited during civil war (d)	0.95	0.226	0	1
Did you join against your will? (d)	0.64	0.481	0	1
Observations	760			

Note: The letter *d* denotes a binary indicator (1 = Yes, 0 = No). All other variables are numbers. Further explanations are provided in the main text.

Table 2.2: Share of UNITA recruits in cells across time and regions

	Date of entry (bin)				
Region	1	2	3	4	p[Means equal within row]
Huambo: North	0.20	0.18	0.42	0.71	0.000
	(0.058)	(0.068)	(0.083)	(0.114)	
Huambo: Center	0.21	0.09	0.11	0.38	0.000
	(0.042)	(0.033)	(0.029)	(0.068)	
Huambo: East	0.36	0.50	0.10	0.38	0.012
	(0.105)	(0.151)	(0.069)	(0.085)	
Huambo: South/West	0.42	0.36	0.57	0.55	0.262
	(0.074)	(0.081)	(0.106)	(0.078)	
Outside Huambo	0.53	0.31	0.39	0.50	0.653
	(0.133)	(0.133)	(0.118)	(0.224)	
p[Means equal within column]	0.018	0.002	0.000	0.093	

Note: Standard errors in parentheses. p-values based on joint tests of means within rows (region) and within columns (date of entry bin). Date of entry-bins are defined by the quartiles of the date of entry-distribution.

Table 2.3: Institutional capacity

	Sample			UNITA - MPLA	
	Full	UNITA	MPLA	Diff	*p*
Extraction					
Food	0.75	0.91	0.68	0.23***	0.00
Taxes (economic activity)	0.32	0.36	0.31	0.05	0.17
Recruitment (consent)	0.71	0.78	0.67	0.11***	0.00
Recruitment (force)	0.74	0.78	0.72	0.06*	0.08
Provision					
Services	0.57	0.51	0.60	−0.09**	0.02
Infrastructure	0.61	0.61	0.61	−0.01	0.85
Conflict resolution	0.51	0.49	0.52	−0.03	0.48
Arms	0.18	0.23	0.16	0.07**	0.02
Protection	0.87	0.81	0.90	−0.09***	0.00

Note: All variables are binary indicators denoting any involvement in a certain practice during military service (1 = Yes, 0 = No). Further explanations are provided in the main text. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 2.4: Unconditional differences in background

	Sample			UNITA - MPLA	
	Full	UNITA	MPLA	Diff	*p*
Core variables					
Household size	7.48	7.35	7.53	−0.19	0.44
Number of rooms	2.71	2.63	2.74	−0.11	0.30
Farmer (d)	0.94	0.95	0.93	0.02	0.27
Land size	3.13	3.06	3.16	−0.10	0.54
Livestock	4.27	3.63	4.53	−0.90	0.16
Radio (d)	0.51	0.47	0.53	−0.06	0.16
HH recruits: all	2.57	2.58	2.56	0.02	0.91
Ever displaced (d)	0.65	0.61	0.66	−0.05	0.18
HH members: Luanda	0.53	0.52	0.53	−0.01	0.95
HH members: killed	0.15	0.15	0.15	0.00	0.98
Church = IECA (d)	0.27	0.25	0.27	−0.02	0.52
Father education (d)	0.57	0.54	0.58	−0.03	0.38
Mother education (d)	0.39	0.37	0.39	−0.02	0.59
Urban/peri-urban (d)	0.45	0.27	0.52	−0.25***	0.00
Auxiliary variables					
Church = Catholic (d)	0.65	0.65	0.64	0.01	0.75
Language: Umbundu (d)	0.98	1.00	0.98	0.02*	0.07
Ethnic group: Ovibundu (d)	0.99	1.00	0.99	0.01	0.38
Father: Ovimbundu (d)	0.98	0.98	0.98	0.00	0.88
Mother: Ovimbundu (d)	0.98	0.99	0.98	0.01	0.42
Housing: hut (d)	0.76	0.79	0.74	0.05	0.14
HH recruits: returned	1.07	1.17	1.03	0.14	0.11
HH recruits: not returned	0.90	0.83	0.93	−0.10	0.30
HH recruits: later	0.60	0.59	0.61	−0.02	0.79

Note: The letter *d* denotes a binary indicator (1 = Yes, 0 = No). All other variables are numbers. Further explanations are provided in the main text. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 2.5: Conditional differences in background

	Joined UNITA						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Household size	−0.001	−0.001	−0.002	−0.001	−0.003	−0.002	−0.003
	(0.814)	(0.807)	(0.703)	(0.801)	(0.602)	(0.746)	(0.647)
Number of rooms	0.001	0.001	0.004	0.007	−0.002	0.002	−0.001
	(0.958)	(0.971)	(0.746)	(0.627)	(0.896)	(0.895)	(0.945)
Farmer (d)	0.036	0.036	0.051	0.069	0.036	0.040	0.061
	(0.512)	(0.692)	(0.394)	(0.309)	(0.595)	(0.555)	(0.379)
Land size	−0.005	−0.005	−0.005	−0.005	−0.003	−0.004	−0.002
	(0.463)	(0.144)	(0.503)	(0.561)	(0.708)	(0.644)	(0.814)
Livestock	−0.002	−0.002	−0.003	−0.001	−0.003	−0.002	−0.002
	(0.307)	(0.342)	(0.166)	(0.489)	(0.162)	(0.238)	(0.346)
Radio (d)	−0.016	−0.016	−0.016	−0.023	−0.021	−0.011	−0.030
	(0.591)	(0.701)	(0.621)	(0.492)	(0.536)	(0.732)	(0.384)
HH recruits: all	0.002	0.002	0.003	0.004	0.004	0.005	0.004
	(0.820)	(0.828)	(0.668)	(0.644)	(0.606)	(0.488)	(0.573)
Ever displaced (d)	−0.037	−0.037	−0.019	−0.034	−0.024	−0.024	−0.043
	(0.274)	(0.279)	(0.577)	(0.326)	(0.488)	(0.489)	(0.226)
HH members: Luanda	−0.018	−0.018	0.005	0.003	0.006	0.005	0.002
	(0.245)	(0.262)	(0.650)	(0.827)	(0.658)	(0.714)	(0.907)
HH members: killed	−0.010	−0.010	−0.013	−0.003	−0.010	−0.011	0.000
	(0.804)	(0.834)	(0.765)	(0.942)	(0.818)	(0.807)	(0.996)
Church = IECA (d)	0.040	0.040	−0.022	−0.024	−0.014	−0.014	−0.018
	(0.256)	(0.392)	(0.550)	(0.521)	(0.695)	(0.700)	(0.641)
Father's schooling (d)	−0.035	−0.035	−0.035	−0.026	−0.035	−0.019	−0.026
	(0.354)	(0.453)	(0.345)	(0.488)	(0.357)	(0.613)	(0.503)
Mother's schooling (d)	−0.011	−0.011	−0.024	−0.005	−0.022	−0.012	−0.004
	(0.739)	(0.649)	(0.514)	(0.901)	(0.558)	(0.756)	(0.910)
Urban/peri-urban (d)						−0.208***	
						(0.000)	
Region	No	No	Yes	No	Yes	No	No
YOE-5	No	No	Yes	Yes	No	No	No
Municipio	Yes	Yes	No	No	No	No	No
YOE	Yes	Yes	No	No	No	No	No
Observations	760	760	760	760	760	760	760
R^2	0.32	0.32	0.11	0.06	0.08	0.06	0.01
Adj R^2	0.24	0.24	0.09	0.03	0.06	0.04	−0.01
p(joint significance)	0.641	0.619	0.737	0.965	0.840	0.978	0.893

Note: The letter *d* denotes a binary indicator (1 = Yes, 0 = No). All other variables are numbers. Further explanations are provided in the main text. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 2.6: Conditional differences in background by date of entry-bin

	Joined UNITA			
	(1) Bin 1	(2) Bin 2	(3) Bin 3	(4) Bin 4
Household size	−0.005	0.014	−0.002	−0.004
	(0.601)	(0.235)	(0.863)	(0.808)
Number of rooms	0.022	−0.007	−0.031	0.024
	(0.359)	(0.799)	(0.226)	(0.528)
Farmer (d)	0.145	−0.070	0.121	−0.170
	(0.239)	(0.636)	(0.290)	(0.372)
Land size	−0.002	−0.014	0.004	−0.017
	(0.912)	(0.463)	(0.753)	(0.389)
Livestock	0.004	0.001	−0.006*	−0.011
	(0.270)	(0.862)	(0.073)	(0.198)
Radio (d)	−0.021	−0.044	0.036	−0.136
	(0.732)	(0.489)	(0.561)	(0.109)
HH recruits: all	0.010	0.017	0.018	−0.040**
	(0.459)	(0.237)	(0.237)	(0.045)
Ever displaced (d)	−0.101	−0.114*	0.032	0.094
	(0.121)	(0.097)	(0.618)	(0.275)
HH members: Luanda	0.028*	−0.029	−0.029	−0.044
	(0.099)	(0.366)	(0.295)	(0.342)
HH members: killed	−0.090	0.097	0.097	0.010
	(0.261)	(0.311)	(0.263)	(0.926)
Church = IECA (d)	−0.065	−0.119	0.053	0.120
	(0.329)	(0.104)	(0.432)	(0.195)
Father education (d)	0.048	0.054	−0.097	−0.109
	(0.508)	(0.462)	(0.148)	(0.225)
Mother education (d)	0.029	−0.058	0.079	−0.123
	(0.697)	(0.424)	(0.240)	(0.164)
Observations	227	175	208	150
R^2	0.05	0.07	0.07	0.13
Adj R^2	−0.00	−0.01	0.01	0.05

Note: The letter *d* denotes a binary indicator (1 = Yes, 0 = No). All other variables are numbers. Further explanations are provided in the main text. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 2.7: Migration

	Full sample		Displaced		Not displaced	
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	Urban	Urban	UNITA	UNITA	UNITA	UNITA
Household size	0.005	0.004	−0.000	−0.000	−0.005	−0.005
	(0.422)	(0.577)	(0.968)	(0.968)	(0.585)	(0.585)
Number of rooms	0.016	0.014	0.015	0.015	−0.021	−0.021
	(0.312)	(0.359)	(0.372)	(0.372)	(0.329)	(0.329)
Land size	−0.008	−0.009	−0.001	−0.001	−0.010	−0.010
	(0.360)	(0.326)	(0.916)	(0.916)	(0.427)	(0.427)
Livestock	−0.002	−0.002	−0.003	−0.003	−0.003	−0.003
	(0.362)	(0.337)	(0.145)	(0.145)	(0.466)	(0.466)
Radio (d)	0.090**	0.081**	−0.000	−0.000	−0.059	−0.059
	(0.015)	(0.030)	(0.992)	(0.992)	(0.309)	(0.309)
HH recruits: all	0.005	0.005	0.002	0.002	0.009	0.009
	(0.546)	(0.575)	(0.803)	(0.803)	(0.531)	(0.531)
HH members: Luanda	0.019	0.015	0.005	0.005	0.025	0.025
	(0.294)	(0.395)	(0.690)	(0.690)	(0.507)	(0.507)
HH members: killed	−0.054	−0.052	−0.068	−0.068	0.073	0.073
	(0.290)	(0.310)	(0.199)	(0.199)	(0.310)	(0.310)
Church = IECA (d)	0.019	0.018	−0.003	−0.003	−0.054	−0.054
	(0.654)	(0.666)	(0.949)	(0.949)	(0.381)	(0.381)
Father education (d)	0.028	0.039	−0.031	−0.031	−0.047	−0.047
	(0.492)	(0.345)	(0.500)	(0.500)	(0.454)	(0.454)
Mother education (d)	−0.042	−0.041	−0.017	−0.017	−0.015	−0.015
	(0.317)	(0.328)	(0.717)	(0.717)	(0.799)	(0.799)
Ever displaced (d)		0.087**				
		(0.022)				
Urban/peri-urban (d)				−0.092*		−0.184**
				(0.090)		(0.024)
Observations	760	760	491	491	269	269
R^2	0.04	0.05	0.14	0.14	0.14	0.14
Adj R^2	0.02	0.03	0.10	0.10	0.07	0.07
p(joint significance)	0.172	0.332	0.806	0.806	0.555	0.555

Note: The letter *d* denotes a binary indicator (1 = Yes, 0 = No). All other variables are numbers. Further explanations are provided in the main text. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 2.8: Sexual violence by armed groups

	Wartime sexual violence		
	(1)	(2)	(3)
UNITA (main)	0.038	0.037	0.033
	(0.225)	(0.154)	(0.253)
Army rank	No	No	Yes
Length of service	No	No	Yes
Pre-service background	No	Yes	Yes
Recruitment region	No	Yes	Yes
Recruitment date quintile	No	Yes	Yes
Mean of dep. var.	0.20	0.20	0.20
Observations	760	760	760
R^2	0.00	0.05	0.06

Note: Further explanations on the included variables are provided in the main text. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 2.9: Regular compensation for military service

	Monetary			Food and clothing		
	(1)	(2)	(3)	(4)	(5)	(6)
UNITA (main)	−0.587***	−0.545***	−0.549***	−0.074*	−0.081***	−0.175***
	(0.000)	(0.000)	(0.000)	(0.060)	(0.003)	(0.000)
Army rank	No	No	Yes	No	No	Yes
Length of service	No	No	Yes	No	No	Yes
Pre-service background	No	Yes	Yes	No	Yes	Yes
Recruitment region	No	Yes	Yes	No	Yes	Yes
Recruitment date quintile	No	Yes	Yes	No	Yes	Yes
Mean of dep. var.	0.48	0.48	0.48	0.46	0.46	0.46
Observations	760	760	760	760	760	760
R^2	0.29	0.34	0.35	0.00	0.13	0.17

Note: Further explanations on the included variables are provided in the main text . Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Chapter 3

War and Local Governance: Evidence from Angolan Veterans

This chapter is based on joint work with Patricia Justino.

It is more important to win the support of the people in the countryside than to take and hold cities... Our first goal has to be to win their allegiance and confidence.

Jonas Savimbi, 1986

3.1 Introduction

What is the effect of war on institutions and economic development? External war is a central explanation for the emergence of effective state institutions and economic prosperity in modern nations (e.g. Tilly, 1975; Besley and Persson, 2009; Gennaioli and Voth, 2015). However, most contemporaneous conflicts are *internal*, in the form of civil wars, which are instead associated with debilitating state institutions (e.g. Bates, 2001; Besley and Persson, 2008; Chowdhury and Murshed, 2013) and economic "development in reverse" (e.g. Collier, 2003; Abadie and Gardeazabal, 2003; Mueller, 2013). At the heart of most existing models of the impact of wars on development is a unitary national government that has the monopoly of force over the entire territory and of investments in state institutions (Besley and Persson, 2008, 2011, 2014). Yet, states affected by internal violent conflicts often lack de facto control over substantial parts of the national territory; multiple ruling authorities exist and governance is regionally fragmented (e.g. Kalyvas, 2006). Under certain conditions, local non-state rulers have (or develop) the institutional capacity to claim authority and legitimacy, and to enact policies of public good provision and taxation in a given territory (Arjona, Kasfir and Mampilly, 2015; Sánchez de la Sierra, 2015). To date, the legacies

of these forms of local institutional change marking conflict zones are largely unknown.

This paper analyzes how local forms of governance that emerge in civil wars affect social behavior and local economic development in the long-term, and introduces, theorizes and tests individual-level mechanisms underpinning such a relationship. Based on survey data from Angola, we show that former soldiers who were more involved in the provision of local public goods and social services to civilians – what we define as 'wartime governance'[1] – during military service are more likely to participate in the collective production of public goods twelve years after the end of the civil war. This result suggests that local collective action in post-conflict societies is endogenous to wartime institutions, policies and experiences, which has important implications for policies aimed at building institutional capacity and social cohesion in these contexts.

Our main treatment variable is the individual exposure of soldiers to local 'wartime governance' by their armed group during wartime military service. We operationalize wartime governance as the local policy choices and practices by a ruling actor, and focus on the provision of goods and services to the governed populace, which is at the heart of 'state-like governance' and an effective way of building legitimacy (Levi, 1989), enhancing local productivity and raising tax revenues (Sánchez de la Sierra, 2015).[2] The delivery of these public goods and services requires institutional capacity by the armed group at two levels. First, its organization requires a certain level of administrative capacity since public good provision is often part of the larger political and military agenda of armed groups. Second, the actual implementation requires large numbers of individuals since the delivery of many local public goods, such as security or infrastructure, are large-scale, collective and labor-intensive projects. The main treatment variable measures involvement as a soldier in the delivery of these goods and services to local civilian populations.

Our main dependent variable is the individual participation of former soldiers in collective public good production in the post-war period. This outcome is central to development outcomes, especially in post-conflict societies, for at least two reasons. First, a key challenge faced by underdeveloped regions is the failure of central governments to deliver public goods. Basic public goods and services are often produced locally, and civic cooperation and participation are thus paramount to their production and delivery. Second, high levels of participation in civic organizations and local public good production may in turn also strengthen the norms of

[1]The focus of the paper and the empirical analysis is on governance by civil war actors. The theoretical and conceptual arguments presented will, however, similarly apply to other forms of armed conflict. 'Wartime governance' refers to local or regional governance performed by an armed actor in zones of armed conflict of any type, and also includes feedback mechanisms by civilians. This definition will be discussed in further detail in Section 3.2 and Appendix A.1.

[2]Our conceptual approach to wartime governance is based on a ruling actor's 'institutional capacity', which constrains its policy choices. This view is conceptually consistent with 'state capacity' constraining the policy choices of an incumbent national actor as in the framework of Besley and Persson (2011).

participatory citizenship and collective institutions, which are vital components of inclusive and cohesive societies.[3]

We distinguish two stages in the post-war production of local public goods: planning and delivery. We measure engagement of ex-soldiers in the planning stage through their participation in deliberative institutions, in the form of community meetings, which are key for organizing collective action in Angola.[4] We assess the involvement of war veterans in actual public goods delivery by observing their participation in local collective initiatives that provide public security. Security is one of the three most fundamental public goods (Hoffman, 2015) and is of particular importance in conflict-affected contexts, where formal justice institutions are severely dysfunctional, the state is limited in reach, and insecurity is pervasive (Bateson, 2013).[5] In practice, informal security institutions play an important and positive role, and participation in these collective organizations is open, voluntary and not compensated materially. Specific functions include preventive patrolling and the resolution of conflicts between villagers.[6] We argue that the good provided is of public nature because it benefits everyone in the village, and participation in these groups is hence a valid measure of being involved in the delivery of a key public good.

Theoretically, it is a priori unclear whether individual exposure to wartime governance increases or decreases long-term civic engagement in public good production. We propose and test four theoretical mechanisms linking individual exposure of former soldiers to wartime governance to their contributions to public good production in the post-conflict period: economic interactions, social attitudes, learning and political preferences.

The empirical analysis of these linkages presents considerable data and identification challenges. First, high quality micro-level data on conflict experiences beyond violence are extremely rare. We have therefore collected primary survey data from 759 Angolan government (MPLA) and rebel (UNITA) veterans of the 1975-2002 Angolan Civil War. The dataset contains detailed information on war and military service experiences, pre-service background characteristics and post-war social, economic and political behaviors in 2014. Second, several unobserved individual-level factors are likely to co-vary simultaneously with involvement in wartime governance and post-war participation in public good production.

[3]See, for instance, Sen (1970), Ostrom (1990), Putnam (1993), Guiso, Sapienza and Zingales (2011); and Alesina and Giuliano (2015).

[4]See Rodella (2010) for a detailed study of social capital at the local level.

[5]In addition, the average level of development – especially in rural areas – remains extremely low in post-war Angola, limiting the capacity and scope for collective action in sectors such as infrastructure or education in many regions. Extensive preliminary and ethnographic research revealed security as a top priority among Angolans and defined this focus of this study.

[6]One potential concern may be that these groups resemble criminal networks, and that participation in them may actually be 'bad'. Yet, these groups are neither organized nor perceived in any way like gangs or militias, and participation is universally viewed positively, as confirmed by qualitative fieldwork.

To establish causality, we use an instrumental variable (IV) strategy based on a natural experiment induced by the Angolan Civil War. The basic logic of exogenous variation in exposure to wartime governance has two parts. First, we argue that soldiers who served in periods in which their army gained (more) new territory, were on average more likely to be (more) exposed to wartime governance. The rationale is as follows: an armed actor with a state-building mission is, on average, more likely to engage and invest (more) in wartime governance in areas that had *not* been under its control in the near past, i.e. after (more) gains of new territory. If the extent of territorial gains varies over time, this means that we expect the extent of public good provision at the armed group level to vary over time. When the armed group wants to meet a higher demand for public good delivery in times of territorial wins, or shortly after, its members consequently need to supply more public goods during these episodes. The literature on the Angolan War describes two temporary episodes when MPLA gained significantly more territory than UNITA, while in the other two periods gains were comparable.

In the second part, we show that individuals were exogenously selected into different levels of territorial gains during their service with an armed group. Based on the compulsory nature of military service in the Angolan War, we argue that joining UNITA in combination with being born 20 years before one of two distinct periods in which UNITA lost great amounts of territory made individuals significantly less likely to be exposed to wartime governance than others with a similar personal background. The identifying assumption is that being selected into this 'penalized' group is not correlated with confounding, unobservable individual traits or experiences. The two main validity threats are endogenous sorting into the penalized group based on confounding pre-service traits and simultaneous selection into receiving violence, which has been linked to pro-social behavior by a growing set of studies (Bauer et al., 2016).

Our main result is that individual exposure of former soldiers to wartime governance significantly increases their engagement in both planning and delivery processes of local public goods production more than a decade after the end of the war. We show that this result is robust to the inclusion of local fixed effects, service and post-service control variables, pre-service characteristics, non-linear model specifications, alternative specifications of the intercorrelations in the error term, alternative measures of exposure to wartime governance responses, and that it is not driven by a single wartime governance component alone. Based on IV-estimates we further argue that this result is not due to correlations with unobserved, confounding pre-service, service and post-service variables, or systematic measurement error. Most importantly, we show that the instrument is not correlated with any of the detailed pre-service background characteristics and service experiences of violence we surveyed.

Additional results reveal that the underlying causal mechanisms differ markedly across processes of planning and delivering local public goods. We find that the positive impact on community-meeting attendance is driven by a shift in individual

political attitudes and preferences, whilst increased individual participation in local security groups is fostered by higher levels of interactions with other members of the armed group (learning mechanism). We find equally positive effects of individual exposure to wartime governance on other forms of local social participation and political attitudes, but we do not find evidence for increased mobilization in wider political processes beyond the local level, such as voting in presidential elections or participating in regional protests. We neither find strong effects on social cooperation within the family. Taken together, we interpret these findings as evidence that exposure to wartime governance may stimulate a lasting interest in and engagement with local politics, governance and collective action.

This study is – to the best of our knowledge – among the first to discuss and quantify legacies of the local institutional changes in civil conflict, but is related to several strands of literature. The paper complements an interdisciplinary literature studying the interrelationships between war, institutions and development. Historical and macroeconomic approaches have focused on mutual reinforcement mechanisms at the *national* level (Tilly, 1975; Olson, 1993; Collier, 2003; Besley and Persson, 2008, 2009, 2010). A recent body of microeconomic studies has studied the consequences of exposure to *combat* and *violence* (Voors et al., 2012; Callen et al., 2014; Bauer et al., 2016; Jha and Wilkinson, 2012; Grossman, Manekin and Miodownik, 2015), but has not been able to address the effects of institutional processes and changes that take place in conflict zones (Blattman and Miguel, 2010; Justino, Brück and Verwimp, 2013; Balcells and Justino, 2014). An emerging literature in political science on 'rebel governance' has started to produce descriptive evidence on the forms of local governance, economies, and institutions that emerge in armed conflict (Mampilly, 2011; Arjona, Kasfir and Mampilly, 2015), but knowledge about the lasting impacts of these forms of governance is hitherto very limited. Furthermore, all of these literatures have struggled to identify and disentangle the endogenous effects of war, institutions and development. We address this challenge by making use of a new instrumental variable strategy and theorizing the causal mechanisms that may shape such relationships.

The paper also advances a large literature on the processes that shape behavior and development. This literature has focused on the historical causes and consequences of behavioral and cultural traits (Putnam, 1993; Guiso, Sapienza and Zingales, 2011; Alesina, Giuliano and Nunn, 2013), and those of formal and informal institutions (North, 1990; Acemoglu, Johnson and Robinson, 2001; Dell, 2010). What is much less understood is how institutions and culture interact (see Alesina and Giuliano, 2015), which is analytically complicated by the fact that traits among a certain population are typically (and often necessarily) elicited in the population's institutional environment, which may have moved *simultaneously* with or been shaped by behavioral traits. To untangle these effects, our paper is methodologically closest to a recent paper by Lowes et al. (2015). Lowes et al. (2015) examine the impacts of exposure to state formation in the Kuba Kingdom by comparing Kuba vs. non-Kuba descendants that reside in locations *outside* the Kuba Kingdom. Our methodological innovation is to study the effects of exposure to institutional

variation on the behavior of exposed individuals in their current locations. Using location fixed effects, we essentially compare the behavior of two individuals that reside in the same location, but were both treated 'somewhere else'.[7]

The findings of the paper also inform policy choices by international donors and national agencies in post-conflict contexts.[8] First, our findings contribute to understanding why individuals and local populations may respond differently to development and governance interventions, and to producing programs that are more tailored to needs based on past experiences.[9] Second, we document a wartime source of pro-social behavior among veterans. This insight challenges some (negative) premises of current reintegration programs with respect to veterans, and should be leveraged by innovative new interventions.

The paper proceeds as follows. Section 3.2 discusses the theoretical mechanisms underpinning the causal link from wartime governance to post-war participation. Section 3.3 describes the Angolan context and the natural experiment exploited for identification. Section 3.4 discusses the survey data, as well as the main specifications and assumptions of the econometric analysis. Section 3.5 presents the main results. Section 3.6 discusses the underlying mechanisms. Section 3.7 reports findings on related social and political outcomes. Section 3.8 concludes and discusses policy implications.

3.2 Theoretical framework

3.2.1 Wartime governance

Descriptive evidence from conflict zones across the world shows that armed actors often provide goods and services to civilians. Armed actors as diverse in nature as

[7]One scenario that would fall outside this logic is a soldier who served (mostly) in the location where he resides today. As military service involved enormous amounts of mobility, we argue that this very unlikely. This intuition is confirmed by retrospective survey-data on the respondent's location at 15 pre-specified, known dates of the war (not reported), which suggests that no soldier served in his home community, and that soldiers spent overall extremely little time 'at home' during military service.

[8]In such contexts, veterans' post-war trajectories are vital elements in the transformation of a conflict-affected to a peaceful state, where former fighters are often considered a primary threat to political stability, social cohesion and economic development (e.g. Blattman, Jamison and Sheridan, 2016). They are therefore a focal point of large-scale development assistance, as in demobilization, disarmament and reintegration programs (DDR).

[9]In particular, participatory development projects have become the centerpiece of recent international assistance programming, and often seek to order to build institutional capacity and development 'from below', by stimulating civic participation and inclusion. Over the past decade, the World Bank has injected more than $85 billion of aid into such projects, but interventions have struggled to enhance participation, and individuals' and communities' responses are far from uniform (Mansuri and Rao, 2012; Casey, Glennerster and Miguel, 2012; Fearon, Humphreys and Weinstein, 2015; Berman, Downey and Felter, 2016). Understanding intrinsic variation in civic participation across individuals is therefore crucial for understanding the impacts of these interventions and improving their effectiveness.

the successful Eritrean People's Liberation Front insurgency, the transnational Islamic State, or bandits in stateless areas in Eastern Congo have been shown to provide public goods and social services to local populations (Mampilly, 2011). What motivates this behavior? A dominant view in the literature is that many armed groups provide do so to motivate individuals to join their organization (Weinstein, 2007; Berman and Laitin, 2008), stimulate economic activity, and be able to raise taxes (Sánchez de la Sierra, 2015). From a political point of view, the capacity to tax hinges on legitimacy, and more generally, it will require more resources and capability to sustain a local monopoly of violence and coerce labor and goods, when the armed actor is not recognized as the legitimate sovereign of the controlled territory. Many armed groups therefore attempt to secure or win the support of local populations by establishing forms of governance (Arjona, 2014), paralleling 'hearts and minds' strategies in counterinsurgency operations (Berman, Shapiro and Felter, 2011).[10]

In theory, these arguments may hold for groups acting in an institutional vacuum. Yet, as in the case of Angola, conflict actors often compete with rival actors for territorial control and regional or national governance, which may (further) increase incentives to provide public goods in controlled territory. The work by Besley and Persson (2011) reiterates previous arguments by historians that external war, or the threat of external war, generally creates a demand for spending in common interest internally, which facilitates investments in state building. While less studied, a similar incentive logic – which we explore through our empirical strategy – may apply to competing actors in internal conflicts.

This raises the question when these competing actors will be more likely to provide public goods. Conflict scholars emphasize the role of territorial control. Controlling territory and engaging with local populations are central objectives of warfare and counterinsurgency (Toft, 2014; Kalyvas, 2006; Arjona, 2014), and armed actors that control territory are much more likely to provide public goods that those without territorial control.[11] We postulate therefore that more *new* territory means more demand for public good provision, especially for armed groups with particular interests in securing legitimacy and voluntary support.[12] As a result, we expect that an armed group is more likely to invest and engage more in public good delivery in times of more territorial gains. If the armed group wants to meet the higher demand for governance from territorial gains, its soldiers need to supply more public goods. Based on this logic, we hypothesize that a soldier who serves in a period when his armed group gains (more) new territory will be more likely to be exposed to (more) wartime governance than an identical soldier who serves at a different time.

[10] At the same time, such public investments are just one of a host of strategies employed by armed groups to legitimize claims and achieve long term political goals, and some groups do not provide any public goods (see e.g. Mampilly, 2011; Stewart, 2016). Yet, providing public goods and services is one particularly dominant strategy and has also been portrayed as the most efficient way to secure voluntary compliance (Levi, 1989).

[11] Stewart (2016) argues that secessionist insurgencies that control territory are 46% more likely to provide public goods to civilians than those that do not.

[12] We look at the absolute extent of wins. The argument may also hold for 'net gains'. A caveat here is that group may also lose territory, in which case the net gain in territory may actually be negative.

3.2.2 Wartime governance and long-run behavior

Why would differences in individual exposure to wartime governance shape long-run differences in socio-political behavior? Based on existing literature, we propose and test four theoretical mechanisms that link individual exposure to wartime governance to contributions to collective public good production in the post-conflict period.

Mechanism 1: Economic interactions. Wartime governance can be understood part of a two-way cooperative, economic interaction between combatants and civilians. Repeated two-way economic interactions with civilians during the war may in turn have a lasting impact on a soldier's willingness to contribute to public good production in the post-war period. From a game theoretical perspective, the essence of these economic interactions may in certain situations be modeled as a Prisoner's Dilemma. In the case of two players, both can choose between cooperation and defection. The payoff will be higher if both cooperate than if both defect. Defecting when the other player cooperates will lead to the maximal payoff, while cooperating when the other player defects will lead to the minimal payoff (e.g. Mas-Colell, Whinston and Green, 1995). In the absence of normative mechanisms of cooperation, natural selection favors defection (Bowles and Gintis, 2011). However, evolutionary game theory has proposed that defection may not occur due to the existence of mechanisms such as direct and indirect reciprocity defined by repeated interactions (Axelrod and Hamilton, 1981). The idea of the 'economic interactions' mechanism is that individual behavior depends on how other individuals in the repeated interactions have behaved towards the individual (direct experience) or towards others in the individual's group (indirect experience). Reciprocal motives then underlie strategic actions as mimicking ('tit-for-tat') or punishing others, which is usually costly (Nowak, 2006; Dreber et al., 2008).

We assume that wartime public good provision is a group policy, so that an individual soldier was usually not able to 'punish' or 'reward' civilians during these repeated interactions based on his beliefs. Two scenarios may shape long-run behavior. First, the soldier may perceive that the armed group offered more to civilians than they received in return; for instance, via taxes or other forms of material support, such as food or shelter. In this case, the soldier's belief in the post-conflict period will be that civilians owe him effort, and he may thus be less more likely to engage in the provision of public goods. Second, the soldier may perceive that the armed group offered less to civilians than they received in return. Hence, the soldier's belief in the post-conflict period will be that he owes civilians effort. In the case of Angola, economic interactions were likely intense as food was scarce, especially in the second half of the war (UNICEF, 1998).[13] It is thus plausible that soldiers will feel that they owe civilians, possibly 'for life', fostering a positive impact on individual engagement in public good provision today. Conversely, if

[13]More than half of survey respondents report that they were starving at critical levels "often" or "very often", which emphasizes the vital importance of economic support by civilians during the war.

soldiers feel that they offered significantly more than they received in return, this logic predicts a negative effect.

Mechanism 2: Social attitudes. Wartime governance can also be modeled as a form of social interaction. A considerable body of research suggests that an important human trait is that of 'in-group bias', i.e. humans are more likely to adopt attitudes that favor other members of their own social reference group (e.g. Goette, Huffman and Meier, 2006). In-group bias has been identified as both a cause and consequence of war, whereby social cooperation within communities tends to strengthen before and after conflict, whilst between-community/group cooperation weakens, leading to forms of 'parochialism' (Bowles, 2006, 2009; Choi and Bowles, 2007; Bauer et al., 2014a). Only few studies, however, have compared groups defined by civilian and combatant status, even though such group divisions may carry over to the post-war period (Bauer, Fiala and Levely, 2014b).

It is possible that repeated, positive social interactions with civilians may *weaken* biased attitudes and increase a soldier's motivation to contribute to a public good that will benefit civilians in the post-war period. As in the post-war period basically all are civilians, such an outcome and disposition is effectively similar then to that of 'collectivism' as opposed to 'individualism' which, in cross-cultural psychology, is a central form of cultural variation (Alesina and Giuliano, 2015; Heine, 2015). Theories of collectivist cultures emphasize the embeddedness of individuals (civilians) in a larger group and acting in the group's interest, in contrast to cultures of individualism (Gorodnichenko and Roland, 2011, 2015). These mechanisms suggest, that positive social interactions with and attitudes toward civilians during wartime may result in more participation by former soldiers in collective institutions that benefit civilian populations in the post-war period.

Mechanism 3: Learning. A growing set of studies provides empirical evidence that systems of cooperation may persist over long periods of time. Much of this literature is motivated by the influential model of 'democratic capital' by Persson and Tabellini (2009), which posits that a polity may accumulate 'experience with democracy' which in turn may set it on paths that make transitions out of democracy less likely. Giuliano and Nunn (2013) present evidence for such an effect at the village level. They document the persistence of village-level traditions of democracy and argue that these may scale-up to the national level. One interpretation of these patterns is through the lens of social learning theory, where individuals acquire behaviors through modeling and reinforcement contingencies in the context of social interactions (Bandura, 1973; Banerjee, 1992; Bikhchandani, Hirshleifer and Welch, 1992). The dominant sociological view of the underlying process emphasizes both normative and non-normative mechanisms. Akers (2011), for instance, distinguishes "the direct association and interaction with others and their conforming or deviant behavior" (behavioral/interactional) and "the different patterns of norms and values to which an individual is exposed through association" (normative mechanism).

Irrespective of the nature of the learning process, this mechanism suggests, then, that 'local governance may beget local governance', leading learning individuals to participate in the production of public good production today. The difference between this mechanism and the social attitudes mechanism above is that the effect here is not a function of attitudes towards other persons, but rather emphasizes the importance of imitation and learning.

Mechanism 4: Political preferences. An alternative and popular interpretation of the persistence of systems of government, and their broader effects, is that individuals' deeper political attitudes and beliefs may be shaped by exposure to forms of governance and be transmitted over time. Such a view is consistent with the nascent literature on endogenous political preferences (e.g. Alesina and Fuchs-Schündeln, 2007; Fuchs-Schündeln and Schündeln, 2015). With respect to statehood, Depetris-Chauvin (2015) provides suggestive evidence that individuals living in regions that were more exposed to historical indigenous state-like structures articulate higher trust in *local* councilors and traditional leaders.[14] Gennaioli and Rainer (2006, 2007) show that forms and quality of local governments are associated with their history of state centralization. Hariri (2012) argues that early pre-colonial forms of statehood outside Europe spurred long-run persistence of traditional, authoritarian rule. This mechanism suggests that engagement in the local provision of public goods as part of a local political system may mold political attitudes and preferences in the long run.

3.3 The Angolan Civil War

Between 1975 and 2002, two highly organized and capable military actors fiercely competed in the Angolan Civil War: the *Movimento Popular de Libertação de Angola (MPLA)* and the *União Nacional para a Independência Total de Angola (UNITA)*. Both organizations had emerged as national movements opposing the Portuguese colonial rule in a war of independence between 1961 and 1974, alongside the *Frente Nacional para a Libertação de Angola (FNLA)*. By the time independence was consolidated in 1975, inter-movement fighting for power had erupted. FNLA was to fold away soon, while MPLA, led by upper-class 'assimilados', seized control of the capital Luanda and became 'the government' of the new Angolan nation. UNITA presented itself as the 'true party of *all* Angolans', seized control of large Southern and Eastern territories, and became 'the rebels'.

Both parties managed to secure strong international allies and consistent access to natural resources. MPLA relied on assistance from Cuba, the Eastern bloc and oil revenues, while UNITA was backed by South Africa, the US and the diamond trade

[14] Notably, Depetris-Chauvin (2015) finds no significant effect on trust in general and in *national* politicians.

(e.g. Guidolin and La Ferrara, 2007; Berman et al., 2016). This Cold War 'proxy-war' lasted until 1991, and was characterized by large-scale front line fighting, including the biggest conventional battle of post-WWII Africa in Kuito Canavale in 1987. The Cold War phase ended in 1991, when a ceasefire was agreed in the *Bicesse Accords*. After abortive elections in 1992, MPLA and UNITA returned to war, now without (overt) support by their Cold War allies. Extremely violent episodes and see-saw battles ensued, only interrupted by a failed peace agreement in 1994. In February 2002, MPLA secured a clear and undisputed victory, when UNITA's leader Jonas Savimbi was assassinated in an ambush. Military operations abruptly ground to a halt and a Memorandum of Understanding was signed in April 2002, to be followed by rapid mass demobilization on both sides. Angola has since recorded more than a decade of absence of large-scale collective violence.

3.3.1 Relevance

Angola's recent history sadly offers the ideal setting for our study. The 27-year war was a long, intense and dynamic case of mass militarization and military competition between two nationalist movements. Both invested heavily in their state-building missions (Pearce, 2011) and UNITA built a 'state in the state' (Roque, 2015). Territorial control was highly volatile, but the competitors were the same two actors throughout the war, both with sufficient capacity to enforce compulsory military service for young men (see Chapter 2 and Spall (2015)). This configuration suggests that the population of former soldiers (from either side) is likely to be very large and to contain substantial variation in individual military experiences.

Huambo province. We focus the study on the Central Highlands and Huambo province ('Huambo' hereafter) for three main reasons.[15] First, this region was at the center of the war. It was in Huambo City, Angola's second largest city, that UNITA proclaimed their own government on the day MPLA declared the independence of Angola, on 11 November 1975. Most parts of the vast Central Highlands changed hands multiple times, which makes it possible to directly compare the members, practices and dynamics of the rival organizations. Second, this region was *not* at the center of the preceding colonial war from 1961 to 1974. Most anti-colonial activity and guerrilla fighting took place near the coast and international borders, and both movements initiated their large-scale activities and mobilization strategies in Huambo only when the civil war started. At the same time, the rushed exodus of the Portuguese administration and its Angolans employees led to a literal collapse of the national state. Thus, the two organizations started their operations in Huambo province essentially in an institutional vacuum. Third, Huambo province is the most densely populated region in Angola, but ethnically homogeneous. While the Angola literature suggests that ethnicity was never at the root of the conflict (e.g. Pearce,

[15]Huambo province is roughly of the size of Switzerland (see map in Figure 2.2 in Chapter 2).

2012), this design allows to rule out potential confounding individual factors related to ethnicity.

Wartime politics. While the war presents a case of brutal and prolonged violence, which especially toward the end of the war included strategic massacres of civilians (Ziemke, 2008), both sides also engaged heavily and systematically with local populations under their control in malign ways. The literature on the Angolan Civil War emphasizes that both actors used elaborate strategies to legitimize their mission, win 'hearts and minds', and establish political systems that resembled state functions and institutions locally (Parsons, 2006; Pearce, 2011). Soldiers were required to help their armed units establish local monopolies of violence, recruit young men for (compulsory) military service, regulate social and economic civilian life and, most importantly for this study, deliver public goods and services, such as protection and infrastructure, which are large-scale collective projects.[16]

Post-war politics. We study the effects of exposure to wartime governance twelve years after the end of the war. As Pearce (2012) notes, the end of the war marked the first time since independence that the government had at least notional control of the entire Angolan territory, including large areas it had not held in a long time (or never).[17] While Angola has experienced more than a decade of enormous economic growth following the end of the war, this boom is almost exclusively based on revenues from crude oil exports. State institutions and their influence remain weak, preventing effective political and economic development (e.g. Maier, 2013).[18] Specifically, regional penetration by the central state remains extremely limited outside the capital and governance in many regions strongly depends on collective cooperation and coordination locally.

Taken together, our setting allows us to investigate the long-run effects of exposure to wartime governance on post-war contributions to local public good production, which is of critical importance to local welfare.

[16]Based on the data described in the next section, Table A.1 presents aggregate data on our main measures of exposure to wartime governance and related forms of interactions with civilians. Each entry denotes the fraction of soldiers that were involved in a given activity. The results reveal that a substantial number of soldiers on both sides experienced these activities. Please note that numbers preceded by an 'A' indicate that the figures and tables are presented in the Appendix.

[17]According to Soares de Oliveira (2013) the government held a mere 20% of its nominal territory during certain periods of the war.

[18]Despite an average annual GDP growth rate of 10.3% between 2000 and 2014, with an all-time high of 23.2 percent in 2007, Angola had the highest under-five mortality rate of all countries in the world in 2014 (WDI, 2015). In addition, the number of NGOs, foreign aid projects and their influence are also extremely small in Angola, as compared to countries of comparable development status (Soares de Oliveira, 2011).

3.3.2 Natural experiment in exposure to wartime governance

In Section 3.2 we hypothesized that a soldier who serves in a period when his armed group gains (more) new territory will be more likely to be exposed to (more) wartime governance than an identical soldier who serves at a different time. We now discuss large-scale territorial changes during the Angolan war and show how individuals were exogenously selected into different levels of territorial gains during their service.

Territorial gains: two periods of large-scale relative gains by MPLA

To explore temporal variation in territorial expansions by the two actors we use the conflict event dataset collected by Ziemke (2008), which includes information on major territorial gains, coded by date and actor. In the plots displayed in Figure 3.1a we observe three types of variation: (i) across actors, (ii) across time (within actors) and (iii) in the difference between actors over time. The data confirm historical evidence of two distinct periods when UNITA managed to capture large parts of territory (Pearce, 2011). The first period includes the first years of the civil war until the early 1980s (time period 'I'), when MPLA was at the brink of losing the war early (Maier, 1997). The second period is a rapid and massive growth of UNITA-held territory, when UNITA surprised MPLA in a large-scale operation shortly after the elections in 1992 (time period 'III'). This period extends until the end of 1994, when a ceasefire was agreed in the *Lusaka Protocol*. A third strong UNITA campaign, following the end of the formal ceasefire in 1998, was quickly and strongly overturned by MPLA, leading to MPLA's final victory in 2002 (time period 'IV').

If our hypothesis is correct, we do not expect much difference in exposure between UNITA and MPLA soldiers serving in periods I and III, due to UNITA's comparable strength in these periods. For periods II and IV, we expect that a UNITA soldier would – on average – be *less* exposed to wartime governance than an MPLA soldier. Figure 3.1b presents local polynomial regressions of the individual wartime governance index on the date of military entry. Due to the limited number of observation per year of entry, the confidence bands are obviously large. Yet, the visual intuition is striking: in periods II and IV the index is consistently higher for MPLA as compared to UNITA in the same period, while in periods I and III this is clearly not the case.

Exogenous selection into treatment

The logic above suggests that the interaction between the army a soldier joined and *when* he joined this army is a source of systematic variation in exposure to wartime governance. To understand this interaction and its exogenous origins better, we now discuss which factors determined which army a soldier joined and when.

Variation 1: Army. The literature on war politics in the Angolan Central Highlands argues *against* strong 'selection' into either side based on conventional, individual factors associated with rebel recruitment or mobilization into armed groups (as e.g. in Weinstein, 2007; Berman and Laitin, 2008). The literature documents that control of local territory during the war shifted frequently and concludes generally that *"a person's first contact with any political formation was as likely to have been with UNITA as with the MPLA"* (Pearce, 2012). Pearce (2009) adds that *"political identity was a matter of necessity rather than of conviction. It is for this reason that I use the word 'adherent' rather than 'supporter' when referring to the people who lived under the control of one or other movement during the war, since 'support' suggests a degree of voluntary affiliation which misrepresents the relationship."* These accounts also show that the armed groups were able to exert strong control over territories and their populations.

Information from our survey (see Section 3.4) on recruitment date by army and region presented in Figure 2.4 is consistent with these findings.[19] For each region, we observe considerable variation over time in how likely, i.e. frequent, it was to join a certain army. There is no region where individuals were consistently more likely to join one army than the other throughout the war. Even though both armies sometimes claimed control over pockets of territory in the same region at the same time, a negative correlation in the distribution over time between the two armies is apparent (conditional on region).

Table A.2 presents estimates from a simple regression of the armed group indicator on a large set of family background characteristics, inspired by previous work on rebel recruitment (e.g. Blattman and Annan, 2010). The results suggest that *no* family background characteristic is a robust predictor of which army a soldier joined.[20] Based on these findings, we conclude that the armed indicator was plausibly exogenously determined.[21]

Variation 2: Date of entry. What determined the *date* of military entry? In state-controlled regions, military service was compulsory by law for men in their late teenage years. UNITA, which effectively built a 'state in the state', also had the capacity to enforce mass enlistment, and we expect the age distribution of UNITA soldiers to be consistent with conscription, and thus similar to that of MPLA soldiers.[22] In particular, for soldiers of either side, date of entry into the armed group

[19]To produce these graphs, we divided the sample into five sub-samples based on the broad region where a soldier was recruited. The regions are the Center, North, West and East of Huambo province, and a fifth category, into which all soldiers are pooled who were recruited outside Huambo province (about 6.5% of the sample). We split each regional sub-sample by which army a soldier joined and plot the army-specific densities of entry date.

[20]See also Table 2.4, Table 2.5, Figure 2.6 and the detailed discussion in Chapter 2. It should be noted that the dependent variables denotes the army the soldier joined when he entered the military for the *first time*. Yet, more than 95% of all sampled veterans joined exactly once.

[21]Yet, we acknowledge that concerns may remain that it was not and discuss these further below.

[22]These arguments are explored in detail in Chapter 2.

and date of birth should be highly correlated. Figure 2.3b shows the distribution of age at entry and reveals the expected concentration of mass entries in late teenage years, consistent with compulsory enlistment. The overall mean age at entry is 19.6 years. Figure A.1 reveals substantial variation in date of birth, and Figure 2.10a confirms that there is a very strong linear relationship between date of birth and date of military entry in both armies. This means that a) date of birth is a robust mean predictor of when the individual entered in the army, and b) we should see a similar pattern in exposure to wartime governance over date of birth as over date of entry (shifted by 20 years, the rounded overall mean of age at entry).

Identifying variation: Army x date of birth. We now consider the interaction of army and date of birth, which we categorize into four 'pooled birth cohort' indicators ('Bin 1'–'Bin 4'), corresponding to the four time periods defined above ('I'–'IV'). The three cut-off points separating bins 1 to 4 are the three cut-off points separating periods I to IV, shifted by exactly 20 years.[23]

Figure 3.1c displays plots of the wartime governance index over date of birth, which reveal similar patterns as for date of entry Figure 3.1b. Being born into bins 2 or 4 involves a clear 'wartime governance penalty' for UNITA soldiers. As before, we observe no or even a slightly reversed pattern for bins 1 or 3. The patterns in these unconditional relationships suggest that the Angolan Civil War created an informative and exogenous source of variation in exposure to wartime governance based on the interaction between the army the individual joined and his year of birth. To be precise, we expect that soldiers who joined UNITA and were born into 'pooled birth cohorts' 2 and 4 were significantly less likely to be exposed to wartime governance, compared to all other soldiers.

The identifying assumption is that the combination of joining UNITA plus being born into bins 2 or 4 is uncorrelated with confounding factors. There is no obvious reason to believe that individuals selected into the 'penalty' group were systematically different in observable or unobservable background characteristics. The first source of variation – being born into bins 2 or 4 – is determined exogenously. The second source of variation – joining UNITA vs. joining MPLA – is also plausibly exogenous as suggested by the discussion above. Econometrically, we exclude the interaction of the army and birth-cohort indicators in the 'second-stage' of IV-estimation while including their main effects. So, even if doubts remained about whether the army assignment was pre-determined, there is no immediate reason why the interaction with being born into bin 2 or 4 (as opposed to not) would have been pre-determined.[24] Similarly, we argue that there is a priori no obvious reason why this interaction would predict or correlate with confounding wartime and post-war variables, and provide econometric evidence in the next section.

[23] As noted above, the 20 year-shift is defined by the (rounded) overall mean of age at entry.

[24] See e.g. Adhvaryu and Nyshadham (2015) for a logically similar argument on the interaction of rainfall and physical distance to health centers.

3.4 Empirical framework

3.4.1 Research design and data

The population of this study are all living males who were ever part of an armed group during the Angolan War and reside in Huambo. The primary survey data we use in this paper is based on a sample of 759 Angolan war veterans from 34 different localities collected by the authors in the Study of Angolan Ex-Combatants (POEMA). The quantitative component of POEMA was supported by an anthropological companion study, which included twelve months of ethnographic fieldwork preceding the survey (Spall, 2015). Qualitative findings were used to assess the validity of our hypotheses, determine their relevance in the local context, refine the survey questionnaire design, interpret quantitative results and explore underlying mechanisms. The survey dataset documents detailed information on war and military service experiences, pre-service background and post-war social, economic and political behaviors twelve years after the end of the war.

The outcome variables of interest capture individual engagement in the production of local public goods. We measure involvement in the planning stage by whether the individual participates in community meetings, and use engagement in local self-security groups as a measure of measure of participation in the delivery of a public good (security). The key treatment variable is a soldier's recalled experience with local governance practices by their armed group(s) during the war. We build an index of wartime governance exposure as the simple average over seven items: provision of services (such as providing access to education), building physical infrastructure (such as schools), provision of arms, help with conflict resolution between villagers (e.g. over land), provision of protection and security, requests by villagers for help with conflict resolution between villagers, and requests by villagers to protect the village.[25] In Section A.1 we provide more detailed information on the sampling strategy, interviews, as well as the motivation and nature of our key measures, and discuss summary statistics (Table A.3), alternatively constructed indices of exposure,[26] and potential issues of recall bias.

[25] This list was inspired by leading accounts of local governance by armed groups (Mampilly, 2011; Arjona, Kasfir and Mampilly, 2015).

[26] As our index is based on frequency measures of exposure, we (carefully) condition on length of service in the standard specifications, which may itself be endogenous. As we will show, the inclusion or exclusion of length of service leaves the main effect unchanged, both in terms of magnitude and statistical significance. Alternative indices include different weighting schemes based on principal component analysis and the method suggested by Anderson (2008) as well as directly adjusting the standard index for length of service.

3.4.2 Econometric specifications

For the baseline estimates we specify linear models where the effect of interest β is estimated by OLS regression:

$$Participation_i = \beta Experience_i + \gamma' X_i + \varepsilon_i \tag{3.4.1}$$

Here, *Participation* is the indicator of contemporaneous individual engagement in collective public good production; *Experience* denotes experience with wartime governance; X is a flexible vector of additional control variables; ε is the error term.

The identifying assumption for a causal interpretation is that the experience with wartime governance and the error term are not correlated, conditional on the control variables we include (conditional independence assumption (CIA)). If the CIA holds, the regression derivative equals the average causal effect (ACE), conditional on this set of controls. If the CIA fails, the equality of the regression derivative and the ACE no longer holds.

We include the following sets of control variables across specifications. First, all specifications include community fixed effects to purge the results from systematic variation across localities. Second, we add pre-military service, and therefore pre-treatment, family background characteristics and pre-treatment region fixed effects to control for pre-existing differences.[27] Third, we sequentially add potentially confounding contemporaneous (socioeconomic) variables that have been linked to engagement in public good production and might simultaneously co-vary with wartime governance experience.[28] Fourth, we carefully explore the effect of controlling for potential wartime confounders, such as experiences of violence, bearing in mind that these may not be determined exogenously themselves.[29]

Even after carefully controlling for these factors, we may measure public good provision during wartime with systematic error or may not be able to control for all individual factors that are associated with participation in both wartime governance and post-war public good production. Either would result in spurious estimates, and we rely on IV estimates to mitigate these concerns.

IV estimation. The instrumental-variables (IV) strategy is based on linear models, as in Equation 3.4.1, where the effect of interest β is estimated by IV/2SLS

[27] Pre-treatment variables were collected based on recall questions included in the survey. The individual reference point was the time just before joining an armed group (for the first time), which respondents remembered very well.

[28] Potential confounders include assets, education, wealth or place of birth (see e.g. Blattman, 2009).

[29] We pay most attention to victimization, i.e. 'receiving violence', which has been linked to post-war pro-social behavior (e.g. Bauer et al., 2016; Bellows and Miguel, 2009; Voors et al., 2012; Cassar, Grosjean and Whitt, 2013). Other potential confounders include perpetration of violence (e.g. Humphreys and Weinstein, 2006), army characteristics (e.g. Akerlof and Kranton, 2000; Chen and Li, 2009), length of service in an armed group (e.g. Gilligan and Samii, 2015; Bauer, Fiala and Levely, 2014b) and pre-service background characteristics (e.g. Weinstein, 2007).

regression. To test the relevance of the instrument(s), we use OLS to estimate δ, as specified in the following 'first-stage' equation:

$$Experience_i = \delta UNITA_i \times Bin_i + \lambda' X_i + \nu_i \tag{3.4.2}$$

Here, *Experience* refers to experience with wartime governance; X is the full vector of control variables, including the main effects of *UNITA* and *Bin(s)*; ν is the error term. The four 'pooled birth cohort'-*Bin* indicators are defined by the date-of-birth cut-offs defined in the previous section. Unless explicitly stated, the first (oldest) bin is the omitted category. As a single instrument is favorable in terms of bias as it is approximately median-unbiased (Hahn and Hausman, 2003; Angrist and Pischke, 2008), we chose our single best instrument for our main specifications, where we interact the *UNITA* dummy with a binary indicator *Bin24*. *Bin24* equals one if the respondent belongs to pooled birth cohort 2 or 4. We then produce IV/2SLS estimates of Equation 3.4.1 using this 'first-stage' equation.

Identification. The identifying assumption of the IV-strategy is that the distribution of the instrument, projected onto included controls, is uncorrelated with the error term in Equation 3.4.1. One way to build confidence in the validity of the instrument is to test if it is correlated with observable pre-service background characteristics using the following specification:

$$UNITA_i \times Bin24_i = \pi' K_i + \nu_i \tag{3.4.3}$$

Here, *UNITA* $\times$ *Bin24* is the interaction the *UNITA* dummy with a binary indicator *Bin24*; K is the vector of pre-service background characteristics; ν is the error term. We expect that no component of π is statistically significant from zero. To explicitly explore whether the 'selection' into UNITA based on family background differs in any systematic way across bins, we also estimate Equation 3.4.3 with *UNITA* as the dependent variable, for *Bin24* observations and others separately. Below, we will also conduct further tests related to the exclusion restriction by estimating Equation 3.4.2 using other war and post-war outcomes as the dependent variable.

3.5 Results

3.5.1 OLS results

We find that more individual exposure to wartime governance is positively associated with participation in both the collective organization and delivery of public goods more than a decade later. Table 3.1 shows strongly significant estimates

from a parsimonious model, with the full wartime governance index as the treatment, no additional control variables and Huber-White standard errors (columns 1 and 4). While index measures are widely used in econometric analysis (e.g. Acemoglu, Johnson and Robinson, 2001; Bellows and Miguel, 2009), they are built upon scales that may not reflect constant effects.[30] Inspired by psychological analyses, which often define "high-intensity" as above the 75th percentile, we divide the sample at the 25th, 50th (median) and 75th percentiles of the distribution of the experience score. Columns 2 and 5 in reveal that for both outcomes the significant effect is driven by the top quartile.[31] The magnitude of its impact remains almost the same if the top quartile is compared to the other quartiles combined (columns 3 and 6). To capture the relevant jump in response, ease interpretation and avoid imposing linearity, we collapse the wartime governance index into a coarse binary indicator, defined by the 75th percentile. We interpret exposure above the 75th percentile as 'high exposure to wartime governance' and use this coarsened treatment variable in the main analysis.[32]

Robustness. In Table 3.2 and 3.3 we show that the positive relationship is robust in terms of magnitude and statistical significance, when we include *comuna* fixed effects, pre-service background variables, a series of other control variables, with two-way clustered standard errors.[33] The sequential inclusion of other (potentially endogenous) war experiences does not change the estimates noticeably. For both outcomes measures, the only variable that appears to affect the magnitude of the coefficient is a measure of how much violence a soldier 'received' (victimization). If the victimization variable is correlated with the error term, and is correlated with our explanatory variable of interest, this may bias the estimate of interest. We consider and address these concerns in Section 3.5.2 to show that victimization is not driving our results.

In Appendix A.2.1, we show that the main result is robust to classical, Huber-White, Moulton and wild cluster bootstrapped standard errors (Table A.4) and non-linear model specifications (Table A.5). We also report that the positive association with post-war participation holds for the continuous wartime governance index, and demonstrate its robustness to three alternatively constructed

[30]For instance, moving from 1 to 2 on an index will likely not have the same effect as moving from 3 to 4.

[31]See Figure A.2 for a graphic illustration of the equivalent regressions using octiles instead.

[32]While we are confident to capture a treatment effect concentrated around the 75th percentile, our estimates may be susceptible to coarsening bias (Marshall, 2016). We address this in the robustness checks and report key results also for the full index measure.

[33]For the main specifications we follow Cameron, Gelbach and Miller (2011) to estimate the standard error. Observations may not be independent within two sets of locations: where individuals reside, and where they were recruited from. We thus estimate robust standard errors clustered by the primary sampling unit (*bairro*) and municipality of recruitment. 'Few cluster' issues and alternative techniques to estimate the standard error are discussed later in this section and in more detail in Appendix A.2.1. As also suggested by Cameron, Gelbach and Miller (2011), we rely on conservative inference and ignore stratification and survey weights (these less conservative results are very similar and available upon request).

indices (Table A.6). Looking at the components of exposure separately, we find markedly moderate intercorrelations (Table A.7), lending further support for our preferred method of index construction, and substantial correlations with post-war participation (Table A.8), suggesting that the index effect is not driven by a single component alone. Yet, the result might still not have a causal interpretation and could be driven by correlations with unobserved, confounding pre-service, service and post-service variables, or systematic measurement error. To mitigate these concerns we rely on IV-estimation, which must be balanced, however, against the inevitable loss of efficiency vis-à-vis OLS. Notably, if exposure to wartime governance is actually not correlated at all with the error term, the asymptotic variance of the IV estimator is always larger than that of the OLS estimator. We discuss the relevance of IV estimation, and the informativeness and validity of the instrument below.

3.5.2 IV estimation

Two primary endogeneity concerns are that the extent of exposure to wartime governance may be a) a function of individual background characteristics and b) correlated with other wartime experiences. For instance, those that deliver wartime governance may be 'specialists' that are better educated or much less involved in battlefield fighting than others. If either has an impact on the outcome variables, the simple OLS estimate will be biased.

Table A.9 compares individual background and military traits of soldiers highly exposed to wartime governance to the traits of those less exposed, based on raw survey data collected for this purpose. The top panel suggests that the two sub-samples do not differ much in terms of background characteristics that could have determined selection in more involvement with delivering wartime governance. The only statistically significant difference is that highly exposed veterans had slightly better schooling. The middle panel presents summary statistics on the armed group and the pooled birth cohort indicators used in the IV-analysis and shows that veterans belonging to the exposed sub-sample are not distributed differently across armed groups and bins than non-exposed counterparts. By contrast, the bottom panel reveals systematic differences in military traits. Exposed soldiers trained slightly longer, were marginally less likely to be an infant (the lowest role in hierarchy), more likely to serve in areas where combat took place and in slightly more battalions.

To explore correlations with experience-based control variables included in Tables 3.2 and 3.3 (violence received, witnessed and perpetrated, and length of service), Figure A.3 presents residual-residual plots, where variation from the full set of covariates is partialled out. The plots confirm that victimization (violence received) is correlated with experience with wartime governance, while all others are not. As a growing body of literature also links 'violence received' to pro-social

behavior (Bauer et al., 2016), we pay particular attention to this variable and address potential issues econometrically in two ways. First, we exclude 'violence received' from the econometric model, shifting it into the error term. If the exclusion restriction holds, the instrumental variable strategy then recovers a causal estimate. Second, as a robustness check, we estimate a model that includes 'violence received' and treats it as endogenous, i.e. we instrument for it.[34]

Informativeness. Table 3.4 presents first-stage and reduced-form results. As expected, the combination of joining UNITA and being born in bins 2 or 4 predicts a substantial decrease in experience with wartime governance (column 1). Following Angrist and Pischke (2008), we choose our single best instrument for our main specifications. We collapse the Bin2- and Bin4-dummy variables into the binary indicator variable 'Bin24', which compares being born in pooled cohorts 2 or 4 to all others, and interact Bin24 with the UNITA dummy variable to form the binary instrumental variable for the main analysis. Columns 2 and 3 reveal that the binary instrument is highly informative, with and without the inclusion of post-treatment controls. The F-statistics of about 72 and 106 suggest that our instrument is not 'weak' and mitigate concerns of associated bias (Staiger and Stock, 1997). The reduced form regressions in columns 4 to 6 demonstrate a robust negative association of the IV with either outcome variable. The coefficients are stable across specifications, and – despite the rich specification and restrictive assumptions – are statistically significant at conventional levels.

Validity: endogenous sorting based on pre-service characteristics. As a first test of the validity of the instrumental variable, Table A.10 compares raw means in individual characteristics between two sub-samples defined by the binary instrument.[35] Similar to the comparisons based on actual exposure to wartime governance in Table A.9, background-related traits do not differ substantially across the two sub-samples, but now individual self-reported military traits do not differ systematically either.[36]

As already discussed in Section 3.3, none of these pre-service characteristics is a robust predictor of which armed group a soldier joined (see Table A.2 for results from multiple, linear regression, and the detailed discussion in Chapter 2). Similarly, Table A.11 shows that none of the pre-service variables is systematically associated with the value of the IV, globally (column 1) and locally (column 2). Dividing the sample into individuals born into pooled year of birth bin 2 or 4 versus all others, we find that the absence of a systematic correlation between background characteristic

[34]The results are discussed in Section 3.5.3 and Appendix A.2.2.

[35]We omit the army and year-of-birth indicators upon which the instrument is built.

[36]Given the number of characteristics, we would expect to see significant differences due to chance. It is hence not surprising that the difference in formal training before treatment is significant. Yet, the difference is only marginally significant in statistical and economic terms.

and army joined holds for both sub-groups separately (columns 3-6).[37]

Validity: simultaneous selection into confounding factors. The main threat to identification with respect to wartime experiences is that the instrument simultaneously selects individuals into confounding experiences of violence, specifically 'violence received'.

Table A.12 presents correlations of the IV with the three general and index-based measures of violence (columns 1-3). The estimates are drawn from the least restrictive models, which include only the main effects of army and birth cohorts as controls and use classical standard errors. For all three general measures of exposure to violence, we find no significant relationships and very low R-squared values.[38] To emphasize that the instrumental variable has very limited predictive power for either outcome, we report p-values in brackets next to the coefficients and observe that no p-value is close to conventional levels of significance (p>.47 for all measures).

To strengthen confidence in the absence of simultaneous selection into confounding experiences of violence, we draw on detailed data on exposure to specific forms of violence. A primary concern may be that gaining territory (and hence more exposure to wartime governance) could also imply more exposure to battlefield fighting, i.e. inter-group violence. A priori it seems unlikely that the identifying variation is correlated with inter-group violence, as this should – on average – affect members of both armed groups equally. The logic of the IV, however, is rooted in the differential impact of having joined UNITA vs MPLA and its evolution over time. A second concern may be that soldiers are simultaneously selected into confounding violent interactions with civilians. While such a selection could be plausible, for soldiers these interactions will predominantly entail perpetrating or witnessing – rather than receiving – violence. Yet, the existing literature emphasizes that receiving as opposed to perpetrating violence is associated with more pro-social behavior (Bauer et al., 2016), which is also consistent with the simple correlations in our data. We therefore trust that it is unlikely that violent interactions with civilians invalidate the IV strategy.

The survey data confirm these intuitions. Figure 3.2 displays running means of exposure to six dominant forms of inter-group and one-sided violence over year of birth. It is apparent that the temporal patterns within and between groups are very different from that of exposure to wartime governance. For instance, the strong 'penalty' in exposure from having joined UNITA out of Bin 2 is not present in any of

[37]We project the armed group and IV indicators onto the full space of pre-service background characteristics – some of which are likely correlated, of course – following previous work on rebel recruitment (e.g. Blattman and Annan, 2010). Unconditional comparisons and Bayesian Model Averaging over the full model space of covariate combinations confirm the results, i.e. no background characteristic is significantly correlated with the armed group outcome. These additional results are available upon request.

[38]More restrictive models with more control variables, fixed effects or clustered errors give very similar results and larger standard errors. These additional results are available upon request.

the variables. As 'violence received' is of particular interest, we additionally regress three specific measures of exposure to battlefield violence on the IV. The coefficients displayed in columns 4-6 of Table A.12, corroborate that violence receive is not systematically correlated with the IV.

3.5.3 IV results

IV- and corresponding OLS-estimates for post-war community meeting attendance and security provision are displayed in Table 3.5. The IV estimates confirm the positive effect of exposure to wartime governance on public good production 12 years after the end of the war. As often the case in cross-sections, the IV estimates exceed the OLS estimates, but are markedly stable across specifications and robust to the inclusion of post-war outcomes.

Robustness. As shown in Table A.13 these results are robust to using the continuous measure of exposure. While the single instrument is preferable in terms of approximate bias, the coefficients are also stable across two alternative, additive instrument specifications for both outcomes (Table A.14). For models in which violence received is included and treated endogenously, the magnitude and statistical significance of the effect of interest does not change noticeably either (Table A.15, A.16). Appendix A.2.2 discusses these robustness tests in more detail. While only suggestive, these results add to recent evidence that casts some doubt on the positive role of the exposure to violence for participation (Adhvaryu and Fenske, 2014; Gáfaro, Ibáñez and Justino, 2014).

3.5.4 Village level

An interesting and important question from a development point of view is whether the behavioral relationships we find scale up to more aggregate levels. Section A.2 presents a simple analysis at the village level. While not statistically significant, the results suggest a similarly positive relationship between (individuals') wartime governance and collective action at the village level (Figure A.5), which further strengthens confidence in the relevance of the underpinning micro-effect.

3.6 Mechanisms

To explore the explanatory power of the theoretical mechanisms introduced in Section 3.2, we primarily rely on a set of auxiliary data on additional wartime

interactions with civilians we collected for this purpose.[39] Results are reported in Table 3.6.

Mechanism 1: Economic interactions. To assess the relevance of the economic interactions mechanism, we analyze whether the magnitude of the effect documented in the sections above depends on productive inputs offered to the group. This supply-side impact is measured by taxes collected from and food and service (voluntarily) delivered by civilians. Armed groups often provide public goods and services in exchange for taxes (Mampilly, 2011; Sánchez de la Sierra, 2015), while food and manpower/labor are the most fundamental resources to run and sustain an armed group. It thus likely that soldiers genuinely appreciated the voluntary supply of productive inputs, as opposed to having to coerce them from civilians.[40] Yet, we find that the impact of exposure to wartime governance is *not* significantly different among those who collected taxes or were offered food more regularly (Panel A). The interaction coefficients are moderate in magnitude and statistically insignificant, and do not support the economic interactions mechanism.

Mechanism 2: Social attitudes. To assess the explanatory power of a change in social attitudes towards civilians, we first study the impact of having attended social events together, such as weddings or sports/games – activities with no or little economic and political meaning but representative of social interactions between combatants and civilians during the war. We further analyze a subjective measure of civilian 'compliance', assessing to what extent civilians were not very cooperative, did not follow rules well or even resisted. The interactive effects are slightly negative for both forms of post-war participation, and marginally significant for local security provision (Panel B). Based on our theory, we would expect a positive rather than a negative impact of higher perceived compliance. In combination with the moderate magnitude of all coefficients, we interpret these weak findings as evidence against a mechanism rooted in a reduction of social bias.

Mechanism 3: Learning. The third theoretical mechanism is based on social learning and emphasizes the collective and interactive aspect of wartime governance. In the behavioral/interactional version of the mechanism, individuals may simply 'imitate' the behavior of their peers, while in the normative form of the mechanism, group- or unit-specific norms may exist that promote behavior related to civilians

[39]Similar to our treatment measures, these variables were elicited via a five-point frequency scale, ranging from "never" (=0) to "extremely often" (=4). We also use additional military and post-service outcomes, which will be explained below. For comparability of effects, all auxiliary variables are standardized to zero mean and unit standard deviation.

[40]Food in particular was often extremely scarce in the Angolan conflict (UNICEF, 1998). In our survey, 53% of respondents report that they were "often" or "very often" "that hungry or thirsty that you ate or drank things you would never have imagined to ever eat or drink." In addition, 39% recall at least one situation where they were "that hungry or thirsty that you thought you might die within the next hour."

and are reinforced by the interaction of its members.[41] If such an effect exists, we would expect it to be stronger among those who a) operated most in combat areas, as these interactions are likely to be particularly important and intense when soldiers' lives are on the line and/or b) still interact a lot with former group comrades in the post-war period.[42] The results suggest that – as predicted by theory – the impact on local security provision is substantially stronger among those that still have strong ties to former comrades today and also among combat area unit veterans (Panel C). Interestingly, we find negligible and not statistically significant heterogeneous impacts on community meeting attendance.

Mechanism 4: Political preferences. The final theoretical mechanism is based on a shift in political preferences and emphasizes the 'political system' aspect of local wartime governance. To test the relevance of this mechanism, we analyze whether the magnitude of the effect depends on the extent to which soldiers taught political ideas and gave strategic instructions to local civilians, such as how to act when a different group seizes control of their village. Panel D reveals that both of these activities increase the effect on participation in meetings significantly. In contrast, we find no economically or statistically significant interaction effects with respect to local security provision.

Summary. Taken together, these results suggest that the positive effects of wartime governance on planning and delivering local public goods mask important differences between the two outcomes when considering the underlying mechanisms. Participation in planning activities seems to be driven largely by social learning processes, whereas participation in the delivery of public goods is shaped by changes in political preferences.

Additional tests. Panel E of Table 3.6 reports additional heterogeneity results to test the validity of these two mechanisms further and examine factors that may exacerbate or weaken their relevance. First, we hypothesize that social learning processes and political preferences may depend on how armed groups ruled civilian populations (rows 1–3). In particular, we expect that promoting behavioral norms to regulate social behavior may be related to either mechanism.[43] The results confirm

[41] This is similar in nature to a normative logic in collective violence, which many fear may spill over into the post-war period and create cycles of violence (Littman and Paluck, 2015). Below, we also discuss a direct measure of the extent of social norms set by the group.

[42] The question used to construct a binary combat unit dummy was: "Did your unit(s) usually operate (more) in combat areas or in non-combat areas?". Factional ties are also commonly assumed to proxy for strength of previous norms in reintegration programming. In Sierra Leone - a roughly comparable context- Humphreys and Weinstein (2006) show that ties are not correlated with economic reintegration which strengthens the proxy assumption. The exact question to assess tie strength was: "How often do you usually meet or spend time with people you met in your faction during the war?".

[43] Question: "How often did your unit impose behavioral rules, e.g. about stealing, crime or violence?".

this and show strong and positive interaction effects on local security provision and attending community meetings. To understand the role of qualitative aspects of the rule by the armed group, we analyzed the impact of relatively more ideological and violent types of rule.[44] The results suggest that neither a more ideological nor a more violent rule are significant sources of heterogeneity.

We hypothesize further that the long-term effect of exposure to wartime governance may be related to the rank and number of battalions served by ex-soldiers (rows 4–5). Having reached a higher rank or having served in more battalions significantly increases the effect of interest on community meeting attendance, but also significantly reduces the effect on local security provision. An interpretation consistent with our two mechanisms is that higher ranks as well as exposure to more (and likely different) types of battalions may propel a change in political preferences, for instance through more experience and insights into group strategies and policies. Yet at the same time, both may crowd out the social learning effect, e.g. through reduced time with other fellow soldiers in higher ranks and through volatility in battalion membership.

Next, we explore whether the impact via either mechanism is weaker for rebel veterans, stronger for longer tenures or dissipates over time (rows 6–8). Being a UNITA veteran may depress the long term impact of wartime governance, if being part of the winning side affects revealing preferences and social behaviors in the post-war period. The negative coefficients reported in row 6 are consistent with a negative impact of being a UNITA veteran, but the effect is not significant. Longer tenures may reinforce both changes in political preferences and mechanisms based on the interactions with other soldiers. We find that more time in the military is indeed associated with a (marginally) stronger effect on community meeting attendance, while the interaction coefficient is small and insignificant for local security provision. We observe that the impact is not significantly weaker among those whose army exit dates back longer, which suggests that the effects of wartime governance on both outcomes are persistent.

As a final test for the political preference mechanism we draw on insights from the nascent literature on the effects of exposure to social, political and economic institutions on the formation and shifts of preferences. An overwhelming literature argues that behavioral parameters are more (or most) likely to change between the ages of 18 and 25 (e.g. Giuliano and Spilimbergo, 2014). We therefore ran sub-sample regressions for individuals that joined the army a) at age 17 or younger, b) between ages 18 and 24 and c) at age 26 or over.[45] As predicted by the literature, the effect of exposure to wartime governance for community attendance is highest in the 18-24 range (Table A.17). The effect on local security provision instead tends to increase with age range rather than concentrate in the 18-24 range.

[44]Questions: "How often did your unit impose rules that had ideological aspects, such as what food to eat and clothes to wear?" and "How often did your unit use violence to impose or enforce rules?".

[45]Using 17-24, 17-25 and 18-25 as middle categories gives nearly identical results.

3.7 Political mobilization and social cooperation

In this section, we explore further implications of wartime governance exposure on other forms of political mobilization and social cooperation using additional data we collected data on a range of related outcomes. Given the lack of reliable policy answers to the challenges of enhancing social cohesion and political stability in post-conflict settings, these implications are of particular interest to development policy. For this purpose, we collapse the outcomes into four main additive and standardized indices (z-scores): political participation and attitudes, protesting, social group participation, and family cooperation.[46]

Political mobilization. Panel A of Table 3.7 reveals that exposure to wartime governance is associated with a strong average increase in political mobilization (column 1). Yet, there is important variation in the effects across different aspects of mobilization. The positive overall effect is driven by a strong increase in political group participation (column 2) and interest in politics (column 5).[47] We find no effects on electoral participation, proxied by registering and voting in national elections (but should notice that around 97% of respondents report to have registered and voted).[48] Looking at governance preferences, about 58% of respondents express that local governance systems set up by armed groups during the war are better than governance by 'normal' state authorities. While exposure to wartime governance significantly increases the probability of favoring such forms of governance, we see no effect on voting against the ruling MPLA in the national elections. Second, we look at political protests as a form of political mobilization and collective action (Tilly and Tarrow, 2015). Before the 2012 elections, several marches against government politics took place in Huambo and Luanda. Media reports describe former combatants as the main social group participating in these marches. The government responded to these demonstrations with repression, including crackdowns, incarcerations and violence (e.g. FT, 2012; ISS, 2012). As reported in Panel B of Table 3.7, we find a positive association between wartime governance exposure and the composite 'protesting index' (column 1), including both attitudes towards and actual participation in protests. The positive overall effect is driven by two variables: strong attitudes that protesters are justified to use violent tactics in political demonstrations (column 5), and that the police are justified to respond violently to protesters (column 6). We find however a weak but negative effect on

[46] Some of these indices were adapted from the pioneering work of Blattman, Fiala and Martinez (2014). We report results on the indices and the full set of components separately.

[47] General interest in politics was measured by the answer on a five-point scale to the (deliberately unspecific) question: "How interested are you in politics?". 'Interest in politics' equals one for replies "Quite interested" and "Very interested," and zero else. Participation in local political groups, was measured by binary indicator of group membership, which equals one for being a member, and zero if not.

[48] These numbers seem very high, yet are comparable to findings in other post-conflict settings (see e.g. Blattman, Fiala and Martinez, 2014).

staying at a (hypothetical) protest that turns violent (column 7).[49] One interpretation of these results is that the exposure to wartime governance shifts a soldier's attitudes toward the political or strategic use of violence, but he does not become more 'prone' to violence in general. In addition, we find that exposure to wartime governance is *not* associated with actual participation in protests, including the marches in Huambo preceding the elections, any other demonstrations in the past year, and hypothetical protests in the future (columns 2 to 4).

We conclude from these results that individual exposure to wartime governance stimulates an interest in and engagement with politics and political collective action at the local level. Yet, it does not appear to affect taking part in actual action at the regional or national levels.[50]

Social cooperation. Panel A of Table 3.8 reveals that the exposure to wartime governance is – on average – also associated with a strong increase in social participation at the local level (column 1). The positive overall effect is driven by a strong increase in participation in social and political groups (columns 2–4). These positive effects stand in deep contrast to the strongly negative effect on participation in religious groups and communities (column 5). Religious communities are key networks and central elements of social capital in most developing countries, and a focus of the anthropological study supporting this project (Spall, 2015). The ethnographic results of this study describe how Angolan soldiers rely on religious engagement and religiousness as *the* primary mechanism of signaling a certain lifestyle and living up to ideals of masculinity and senior male status. In a framework of pro-social behavior, these results clearly emphasize the salient role of social reputation, consistent with arguments in the theoretical literature in economics (e.g. Bénabou and Tirole, 2006). Their motivation to participate in religious organizations could hence be largely driven by reputational concerns. Conversely, Spall (2015) suggests that the 'social benefits' of participation in non-religious groups are distinctly lower, and therefore arguably less strategic in terms of social reputation. These insights lead us to conclude that the dynamics of wartime governance may reduce the incentives (or needs) of soldiers' incentives to rely on religious participation as a strategic means to improve social integration. Finally, looking at the family level, Panel B in Table 3.8 documents weak and slightly *negative* effects of wartime governance exposure on cooperation within the family (column 1). The negative association appears to be driven by a reduction in caring about the spouse and children (column 4). The results also reveal a similarly negative but insignificant effect on harmony, and no effects on living together or being married (columns 2,3

[49] Question: "Imagine that you are at a protest and it turns violent, would you stay or leave?".

[50] This insight complements findings by Depetris-Chauvin (2015) who documents that a positive effect of a local history of indigenous state-like structures on trust in policy actors is confined to the local level. Individuals from regions with a long history of statehood trust local policymakers more, but there is no effect on trust in national policymakers.

and 5).[51] This suggests that the positive effects of wartime governance exposure on social participation do not include family relationships. One potential explanation is that being more active in the community may crowd out kin relationships, for instance via time constraints.

Taken together, the results on social participation strengthen our finding that individual exposure to wartime governance can foster social engagement, even beyond contributions to local public goods. Interestingly, these positive effects are not reflected in within-family cooperation – like the political effects, the social effects are confined to the community-level.

3.8 Conclusion

This paper provides a first stepping stone towards a more in-depth understanding of institutional change during civil war and the long-term consequences for behavior and local development. The key result is that former soldiers who were more involved in local governance with their armed groups are significantly more likely to participate in the collective production of local public goods more than a decade after the end of the war. The empirical analysis relies on primary survey data from Angolan ex-combatants and exploits the Angolan Civil War as a natural experiment to establish that the documented effect has a causal interpretation. Further analysis suggests that the underlying causal mechanisms differ markedly across outcomes of planning and delivering local public goods. We find that a shift in political preferences increases community-meeting attendance, while social learning fosters participation in local security provision. We find similar impacts of individual exposure to wartime governance on other forms of local social participation and political attitudes, but do not find any evidence for similar effects on mobilization in a wider political sense (in the form of voting in national elections or regional protesting), or on social cooperation within the family. We interpret these findings as evidence that exposure to wartime governance stimulates lasting interests and participation in *local* politics, governance, and collective action.

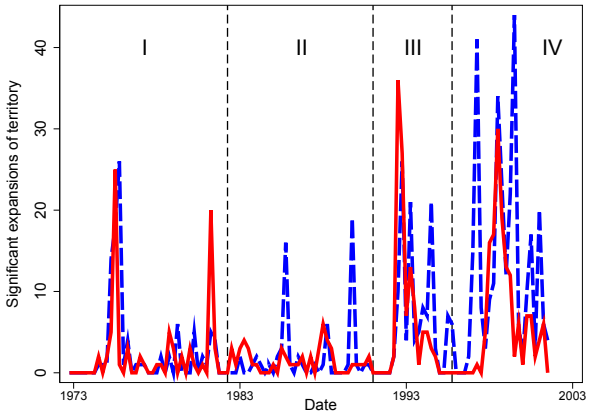
While the empirical analysis is focused on the case of Angola, we draw on the 'rebel governance' literature to argue that many aspects of our treatment – wartime governance – are qualitatively similar in many conflict zones across the world. We therefore expect our findings to have two valid and important policy implications for state-building and local development in post-conflict societies. First, policy designs often emphasize the importance of breaking ties between former fighters, assuming that clustering may fuel the risk of recurring violence. We document a source of pro-social behavior among veterans and find that remaining factional ties may in fact reinforce the positive impact on contributing to local public goods and services.

[51] The caring and harmony variables are subjective four-point measures reported by the veteran's partner in a separate interview, and are only included for those respondents that have a partner.

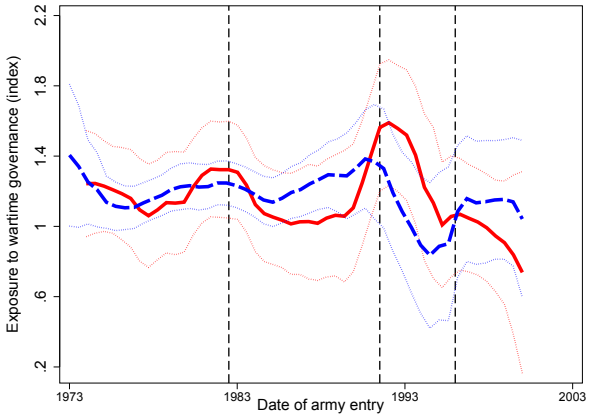
Second, community-driven development programs and initiatives to build national state capacity 'from the bottom' have struggled to deliver an impact, with results varying substantially across contexts. Our findings can help to understand why certain groups respond differently to interventions related to governance and participation. Conceptually, we show that some of the variation may originate from systematic differences in experiences related to forms of institutional change that took place during armed conflict. Specifically, civil wars may spur institution-building processes that foster lasting preferences for *local* and *collective* forms of governance, possibly at the expense of national governance. Hence, settings characterized by strong institutional change during conflict may take longer to build strong and cohesive national states in the post-conflict period.

Figure 3.1: First-stage mechanism

(a) Event data: Significant expansions of territory



(b) Survey data: Exposure to wartime governance by date of entry



(c) Survey data: Exposure to wartime governance by date of birth

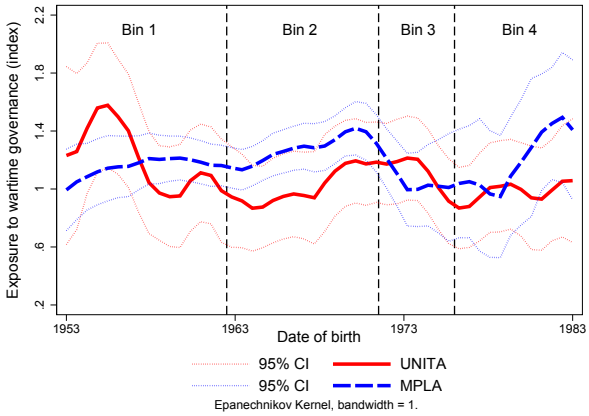
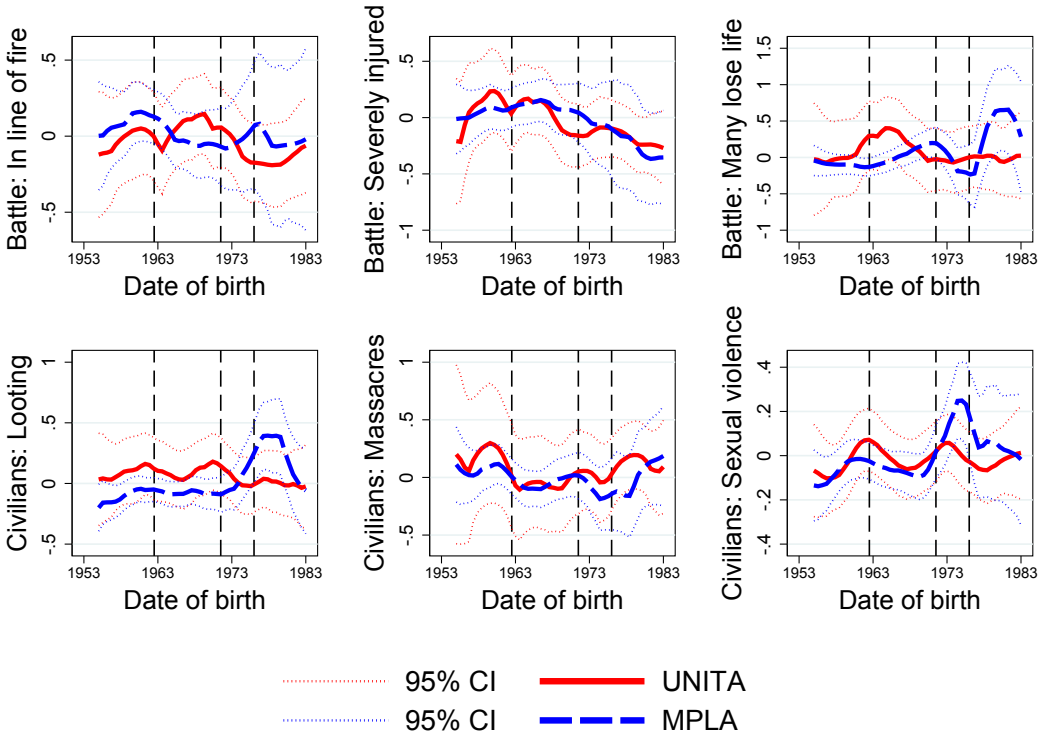


Figure 3.2: Exclusion restriction: intergroup violence and violence against civilians



Epanechnikov kernel, bandwidth = 1.5.

Table 3.1: Wartime governance and participation in public good production (OLS)

	Community Meetings			Local Security		
	(1)	(2)	(3)	(4)	(5)	(6)
Wartime governance (index)	0.080***			0.071***		
	(0.019)			(0.011)		
—WG index quartile 2		0.011			−0.015	
		(0.041)			(0.024)	
—WG index quartile 3		0.028			−0.023	
		(0.040)			(0.023)	
—WG index quartile 4 (top)		0.183***	0.171***		0.142***	0.153***
		(0.042)	(0.036)		(0.024)	(0.021)
Mean dep.var.	0.224	0.224	0.224	0.064	0.064	0.064
Observations	760	760	760	760	760	760

Note: WG index quartile x: binary indicator of quartile x (1 = Yes, 0 = No), defined by the 25th, 50th, and 75th percentiles of the distribution of the wartime governance index. The reference category is quartile 1 (bottom). Significance levels:* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Classical standard errors in parentheses.

Table 3.2: Community meeting attendance: robustness to control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
WG high	0.173***	0.173***	0.173***	0.155***	0.173***	0.168***	0.166***		0.135***
	(0.043)	(0.042)	(0.043)	(0.047)	(0.043)	(0.040)	(0.038)		(0.041)
Length of service		0.003							0.000
		(0.003)							(0.002)
Violence witnessed			−0.013						−0.030
			(0.016)						(0.022)
Violence received				0.037				0.061**	0.056**
				(0.028)				(0.025)	(0.026)
Violence perpetrated					−0.004				−0.009
					(0.011)				(0.013)
Radio						0.044			0.042
						(0.032)			(0.036)
Wealth						0.022***			0.020***
						(0.006)			(0.005)
Education						0.087***			0.084***
						(0.029)			(0.031)
Born here						−0.029			−0.017
						(0.022)			(0.020)
Age							0.006		−0.001
							(0.018)		(0.014)
Age^2							−0.000		−0.000
							(0.000)		(0.000)
UNITA (main)							0.013		0.022
							(0.046)		(0.046)
YOB-Bin 2							−0.136***		−0.126***
							(0.030)		(0.034)
YOB-Bin 3							−0.200***		−0.199***
							(0.064)		(0.067)
YOB-Bin 4							−0.214***		−0.228***
							(0.077)		(0.073)
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760	760
R^2	0.09	0.09	0.09	0.09	0.09	0.11	0.11	0.07	0.14

Note: *WG high*: binary indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: binary indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin x*: binary indicator of year of birth bin x (1 = Yes, 0 = No). The sample is grouped into four bins (1 − 4), as explained in the text. *YOB-Bin 1* is the oldest group and the reference bin, *YOB-Bin 4* is the youngest group. Note that *age* in this case captures within *YOB-Bin* variation. *Pre-Controls*: vector of eight pre-service family background characteristics. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses, two-way clustered by *bairro (= PSU)* and municipality of recruitment.

Table 3.3: Local security provision: robustness to control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
WG high	0.153***	0.153***	0.153***	0.141***	0.153***	0.153***	0.150***		0.135***
	(0.033)	(0.033)	(0.033)	(0.035)	(0.033)	(0.033)	(0.034)		(0.037)
Length of service		0.002							0.002
		(0.002)							(0.002)
Violence witnessed			−0.002						−0.019
			(0.012)						(0.013)
Violence received				0.025				0.047***	0.033**
				(0.017)				(0.017)	(0.016)
Violence perpetrated					0.003				0.003
					(0.005)				(0.006)
Radio						−0.012			−0.010
						(0.032)			(0.032)
Wealth						0.003			0.002
						(0.006)			(0.006)
Education						−0.004			−0.007
						(0.019)			(0.017)
Born here						−0.014			−0.011
						(0.027)			(0.024)
Age							−0.002		−0.003
							(0.015)		(0.013)
Age^2							0.000		0.000
							(0.000)		(0.000)
UNITA (main)							−0.008		−0.015
							(0.022)		(0.026)
YOB-Bin 2							−0.006		−0.005
							(0.018)		(0.017)
YOB-Bin 3							−0.037		−0.033
							(0.052)		(0.051)
YOB-Bin 4							−0.038		−0.034
							(0.060)		(0.058)
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760	760
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.13	0.07	0.14

Note: *WG high*: binary indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: binary indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin x*: binary indicator of year of birth bin (1 = Yes, 0 = No). The sample is grouped into four bins (1 − 4), as explained in the text. *YOB-Bin 1* is the oldest group and the reference bin, YOB-Bin 4 is the youngest group. Note that *age* in this case captures within *YOB-Bin* variation. *Pre-Controls*: vector of eight pre-service family background characteristics. *Post-Location*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table 3.4: First-stage and reduced form

	First-stage			Reduced form			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Public good high			Community meetings		Local security	
UNITA x YOB-Bin 2	−0.237***						
	(0.064)						
UNITA x YOB-Bin 3	−0.027						
	(0.095)						
UNITA x YOB-Bin 4	−0.296***						
	(0.043)						
UNITA x YOB-Bin2/4		−0.246***	−0.237***	−0.072**	−0.082***	−0.054**	−0.053**
		(0.029)	(0.023)	(0.034)	(0.029)	(0.024)	(0.025)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Controls	No	No	Yes	No	Yes	No	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	No	No	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760
R^2	0.05	0.05	0.07	0.10	0.12	0.07	0.07
F(UNITA x COB = 0)	13.70	71.92	106.43				

Note: *UNITA (main)*: binary indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin x*: binary indicator of year of birth bin (1 = Yes, 0 = No). The sample is grouped into four bins (1 − 4), as explained in the text. *YOB-Bin 1* is the oldest group and the reference bin, YOB-Bin 4 is the youngest group. *YOB-Bin 2/4* collapses *YOB-Bin 2* and *YOB-Bin 4* into a joint bin. *YOB-Bin*: full set of binary indicators of year of birth bin (1 = Yes, 0 = No). *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses; in columns 1 and 2 clustered at the pre-service location level, in columns 3–7 two-way clustered by *bairro* of residence and municipality of recruitment.

Table 3.5: Participation in local public good production (IV)

	Community Meetings				Local Security			
	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) OLS	(6) OLS	(7) IV	(8) IV
WG high	0.161***	0.154***	0.302**	0.344***	0.152***	0.152***	0.225**	0.221**
	(0.041)	(0.039)	(0.134)	(0.122)	(0.034)	(0.035)	(0.093)	(0.102)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Controls	No	Yes	No	Yes	No	Yes	No	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760
R^2	0.12	0.14	0.10	0.11	0.13	0.13	0.11	0.12

Note: *WG high*: binary indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: binary indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of binary indicators of year of birth bin (1 = Yes, 0 = No). *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table 3.6: Mechanisms of impact

	Community meetings	Local security
	(1)	(2)
Panel A: Economic interactions		
WG high x Voluntary supply	0.017	−0.051
	(0.070)	(0.053)
WG high x Collected taxes	−0.014	0.031
	(0.029)	(0.022)
Panel B: Social attitudes		
WG high x Socializing	0.001	0.021
	(0.029)	(0.031)
WG high x Compliance	−0.035*	−0.038
	(0.021)	(0.031)
Panel C: Learning		
WG high x Factional ties	−0.005	0.075***
	(0.042)	(0.019)
WG high x Combat areas	0.023	0.049**
	(0.033)	(0.021)
Panel D: Political preferences		
WG high x Taught political ideas	0.052**	0.007
	(0.021)	(0.039)
WG high x Strategic instructions	0.062***	0.013
	(0.021)	(0.016)
Panel E: Additional tests		
WG high x Social regulation	0.082***	0.083***
	(0.033)	(0.029)
WG high x Ideological rule	−0.006	−0.015
	(0.035)	(0.040)
WG high x Violent rule	0.034	0.024
	(0.038)	(0.026)
WG high x Rank	0.085***	−0.038**
	(0.028)	(0.019)
WG high x No of battallions	0.063***	−0.021***
	(0.023)	(0.007)
WG high x UNITA	−0.013	−0.033
	(0.044)	(0.027)
WG high x Time in military	0.066*	−0.007
	(0.036)	(0.026)
WG high x Time since left military	−0.014	0.024
	(0.036)	(0.018)

Note: Each cell coefficient from a separate regression; model specification as in column 1 of Table 5 plus the main effect of the interacted variable. All interacted variables are standardized to zero mean and unit standard deviation. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table 3.7: Political mobilization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Politics							
	All	Participation			Attitudes		
	Index z-score	Political group	Election: registered	Election: voted	Interest in politics	Wartime governance	Did NOT vote MPLA
WG high	0.540***	0.165***	−0.000	0.014	0.300***	0.085**	−0.013
	(0.088)	(0.032)	(0.008)	(0.011)	(0.050)	(0.033)	(0.020)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep.var.	0.075	0.208	0.979	0.971	0.343	0.579	0.219
Observations	760	760	760	759	760	760	666
Panel B: Protests							
	All	Participation			Attitudes		
	Index z-score	Huambo	Other	Future	Violence protesters	Violence police	Violence stay
WG high	0.255***	−0.006	0.012	0.024	0.155***	0.176***	−0.050*
	(0.091)	(0.012)	(0.021)	(0.040)	(0.035)	(0.037)	(0.028)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep.var.	0.015	0.050	0.034	0.175	0.206	0.112	0.152
Observations	760	744	759	747	728	734	693

Note: The z-score in column 1 is constructed by adding up the values of the binary indicators in columns 2-7, and standardizing the sum. *Std. controls*: Same specification as in column 1 of Table 5. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table 3.8: Social participation and cooperation

	(1)	(2)	(3)	(4)	(5)
Panel A: Groups					
	Index z-score	Cultural	Spiritual	Sports	Religious
WG high	0.441***	0.198***	0.080**	0.093***	−0.065***
	(0.125)	(0.032)	(0.037)	(0.036)	(0.019)
Std. controls	Yes	Yes	Yes	Yes	Yes
Mean dep.var.	0.023	0.064	0.020	0.067	0.475
Observations	760	760	760	760	760
Panel B: Family					
	Index z-score	Lives with partner	Married	Caring z-score	Harmony z-score
WG high	−0.179**	0.014	−0.046	−0.273**	−0.148
	(0.070)	(0.014)	(0.030)	(0.118)	(0.092)
Std. controls	Yes	Yes	Yes	Yes	Yes
Mean dep.var.	−0.004	0.954	0.408	−0.006	−0.005
Observations	760	760	760	566	578

Note: Panel A: The z-score in column 1 is constructed by adding up the values of the binary indicators in columns 2-5, and standardizing the sum. Panel B: The z-score in column 1 is constructed by standardizing the values of the binary indicators in columns 2-3, adding these values to those of the standardized indicators in columns 4-5, and standardizing the sum. *Std. controls*: Same specification as in column 1 of Table 5. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Chapter 4

Long-run Individual-level Origins of Domestic Violence

This chapter is based on joint work with Tilman Brück.

4.1 Introduction

Why do so many men harm their intimate partners? Globally, one in three women experience domestic violence during their lifetime (WHO, 2013; Devries et al., 2013), creating enormous risks for victims and future generations (Pollak, 2004; Aizer, 2011; Sabia, Dills and DeSimone, 2013).[1] The welfare cost of domestic violence is estimated at around 5% of global GDP, or about five times that of wars (Fearon and Hoeffler, 2014). However, identifying reliable policies of preventing and reducing domestic violence is proving difficult (Iyengar, 2009; Bobonis, González-Brenes and Castro, 2013; Ellsberg et al., 2015). A central limitation is that intrapersonal factors that lead men to commit violent acts against intimate partners are not well understood.

Across disciplines, most research traces the origins of domestic violence to the contemporaneous social, cultural and family environments. The dominant approach attributes domestic violence to prevailing cultural norms, predominantly those of gender identity and relations (e.g. Bloch and Rao, 2002; Dagirmanjian et al., 2016). In economics and adjacent disciplines, the main alternative approach models domestic abuse as part of an intra-household bargaining process between partners (Farmer and Tiefenthaler, 1996, 1997; Aizer, 2010; Anderberg et al., 2016). By contrast,

[1] While the term "domestic violenc" generally includes all violence between individuals within households, we will focus on violence against women by an intimate partner and refer to intimate partner violence (IPV), spousal violence and domestic violence interchangeably. As common in academic and public debates, we focus on physical and sexual domestic violence in the baseline analysis, but also discuss emotional violence briefly.

psychologically motivated perspectives emphasize the relevance of intrapersonal and situational factors for violent behaviors. Yet, even psychologists and public health scholars lament that psychological "theory and research on relationship violence remain uncohesive" (Berscheid and Regan, 2005, 52) and that "there is little conceptual organization regarding how and why risk factors influence IPV" (DeWall, Anderson and Bushman, 2011, 247). Causally identified impacts of variation in these factors are particularly rare across disciplines. Card and Dahl (2011) formally introduce such a perspective into the economics literature, and provide a model and empirical evidence for the causal impact of short-term emotional cues on domestic abuse following upset losses by sports teams.

This paper studies the causal impact of individual experiences by men on long-term violent behavior against female intimate partners. Specifically, we focus on the exposure to sexual violence by armed groups against women in times of war and ask how this exposure affects a former soldier's propensity to commit intimate partner violence more than a decade after the end of the war. By 'exposure', we mean that a soldier experienced situations where he was either witnessing or perpetrating these acts, as captured by his survey responses. For ethical and legal reasons, the questions did not probe the degree of active or voluntary participation. For further details, see Section B.1.

We study the impact of exposure to wartime sexual violence for two main reasons. First, perpetrating or witnessing violence is generally correlated with engaging in violent behavior in the future – 'violence begets violence' (e.g. Pollak, 2004; Littman and Paluck, 2015). Second, sexual violence is among the most intense and recurring forms of gender-based violence and war atrocities, affecting millions of people across the world. Starting with events in Bosnia and Rwanda, awareness for wartime sexual violence increased rapidly, and United Nations Security Council Resolution 1820 recognizes that "rape and other forms of sexual violence can constitute war crimes, crimes against humanity or a constitutive act with respect to genocide" (UN, 2008). Nonetheless, armed groups around the world continue to perpetrate sexual violence, including contexts as diverse as Liberia, Eastern Congo, Colombia, Timor–Leste, Pakistan, or Iraq and Syria (Cohen, Green and Wood, 2013).

Theoretically, it is a priori unclear whether a man's exposure to sexual violence against women increases or decreases his long-run propensity to commit domestic violence. We propose and test five theoretical mechanisms. These include two 'indirect' mechanisms, which operate via the man's interactions with the socio-cultural environment today: traditional norms of gender identity and relations and intra-household bargaining. In addition, we specify three 'direct' mechanisms, which operate via processes internal to the exposed individual: violent models, distress, and violent practice against women.

Testing a causal relationship between exposure to wartime sexual violence and post-war domestic violence presents two empirical challenges. First, high-quality micro-data on wartime sexual violence is scant, as both measuring exposure to these

acts quantitatively and collecting survey data is difficult. We therefore collected primary survey data from 578 Angolan government and rebel veterans of the 1975-2002 Angolan Civil War and their intimate partners.[2] The dataset contains detailed information on war and military service experiences, pre-service background and post-war social, economic and intra-household behaviors twelve years after the end of the war. To reduce bias from self-reporting committed acts of spousal abuse, we rely on reports on acts of domestic violence by the veteran's partner, collected in separate and private interviews with a female enumerator, and based on standard Demographic and Health Survey (DHS) protocols and survey instruments. Second, several unobserved individual-level factors are likely to co-vary simultaneously with exposure to wartime exposure to sexual violence and domestic violence today, or respondents may misreport in a non-random way. To overcome this challenge and to establish causality, we use an instrumental variable (IV) strategy based on a natural experiment induced by the Angolan Civil War.

Angola's recent history offers a suitable setting for our study. The civil war included 27 years of mass militarization and intense military competition between two rival movements, the *Movimento Popular de Libertação de Angola* (MPLA) government and the *União Nacional para a Independência Total de Angola* (UNITA) rebels. Both actors engaged heavily with civilians in violent and non-violent ways (Pearce, 2011; Ziemke, 2008), including sexual abuse against women (HRW, 2001). In post-war Angola, domestic violence is now increasingly recognized by the national government as a widespread and grave violation of human rights (e.g. UN, 2013).[3]

In the IV-strategy, the logic of exogenous variation in exposure to sexual violence by armed groups proceeds in two steps. First, we exploit the basic insight that (especially high-capacity) armed groups often exhibit sexual violence against civilians episodically, rather than continuously. We thus hypothesize that soldiers who served in periods in which their army engaged in more sexual violence were, on average, more likely to be exposed to wartime sexual violence. In Angola, two distinct episodes of increased MPLA operations against civilians occurred that systematically included sexual violence. In the second step, we recognize that individuals were exogenously selected into these armed group-specific distinct episodes. Based on the compulsory nature of military service in the Angolan War, we argue that a soldier who joined MPLA and *in addition* was born about 20 years before one of the two distinct time periods, was significantly more likely to be exposed to sexual violence during his tenure.[4] The identifying statistical assumption is that this

[2]Here, we only retain respondents with co-habitant intimate partners on domestic violence reported in a private interview. We discuss selection into missing data further below.

[3]The government's concern over gender-based violence resulted in the Angolan "Law against Domestic Violence," No. 25/11, which was unanimously approved by the Angolan Parliament on July 14, 2011. Also approved for undergoing legal formalities were a "Plan for Combating Domestic Violence", including a "Family Action Plan" and "National Gender Police." A specific aim of the initiative is "to criminalize the aggressors [...], thus making violence a matter of public concern". These developments could potentially influence our study. Yet, extensive pre-survey fieldwork in 2013 revealed that informal authorities and the general public were not yet aware of these innovations.

[4]We use the 20 year-difference, as the overall mean age at recruitment was narrowly centered

interaction is a) a predictor of *individual* exposure to sexual violence (informative) and b) uncorrelated with (unobserved) confounding individual traits or experiences (valid).

Our central result is that exposure to wartime sexual violence against women significantly increases a soldier's propensity to commit domestic violence more than a decade after the end of the war. We provide evidence that this effect is not driven by local effects, omitted individual variables, measurement error, linear model specification, mis-specified intercorrelations in the error term or the choice of our preferred instrumental variable specification. We argue that the long-lasting impact on perpetrating domestic violence is caused by breaking a psychological barrier to violence against women, conditioning soldiers into 'gender-based violent practice'. We find no support for alternative theories, based on a reduction in economic bargaining power, living up to norms of gender, socialization, and distress. Our results are consistent with an interpretation of domestic violence as 'expressive' behavior, where men intrinsically value the expression of violence and/or it arises as an unintended outcome.

Our theoretical arguments and empirical findings contribute to three literatures. First, we extend the literature on domestic violence.[5] In addition to our contribution to understanding the origins of domestic violence discussed above, our findings also inform debates about the behavioral nature of acts of domestic violence. Domestic violence is predominantly assumed to be 'instrumental' behavior and used to control the partner, send signals, or extract resources, among other objectives (e.g. Bloch and Rao, 2002; Bobonis, González-Brenes and Castro, 2013). Recent contributions – including modern bargaining models – increasingly advance that domestic violence may be expressive behavior (e.g. Aizer, 2010; Card and Dahl, 2011). Our findings provide supporting evidence that domestic violence may indeed be expressive behavior, originating from individual experiences in the past.

Second, our paper also adds to the literature on the existence and nature of cycles of violence.[6] At the individual level, cycles of violence present a puzzle, as a) exposure to violence is aversive and distressing for most people, and yet does it appear to make individuals more prone to violence in the future (Littman and Paluck, 2015), and b) exposure to war violence sometimes seems to make individuals to

around 19.5 years and stable over time.

[5]The bulk of existing studies in economics has focused on the empirical relations of male unemployment and female empowerment with domestic violence. The evidence is markedly mixed (see e.g. Anderberg et al., 2016; Hidrobo, Peterman and Heise, 2016). One potential explanation for the inconclusive evidence suggested by recent work is that relevant contemporaneous norms may a) have deep long-term societal origins (Tur-Prats, 2015) and b) shape intimate partner violence crucially by *interacting* with female intra-household power status (Alesina, Brioschi and La Ferrara, 2016; Tur-Prats, 2016).

[6]For a review of the literatures on intergenerational transmission of violence and the relationship between the exposure to violence at a young age and perpetrating violence and violent crime later in life, see Widom and Wilson (2015). For a review of the extensive macroeconomic literature on cycles of violence and violent conflict, see Blattman and Miguel (2010).

behave more – rather than less – pro-socially (Bauer et al., 2016). Littman and Paluck (2015) emphasize the need to develop a better understanding of how collectives, and armed groups in particular, trigger cycles of violence at the individual level. Studying the links between initial experiences of violence and violent behavior in the future is complicated, however, by the fact that violent experiences in the first place will typically not occur randomly, which severely limits the availability of causal evidence (Widom and Wilson, 2015).[7] We use an original design to test the causal, long-term impact of previous exposure of violence on violent behavior today, as reported by a different person (the intimate partner). Specifically, we establish a robust link from wartime sexual violence by armed groups to post-war domestic violence by ex-combatants, guided by theory from multiple disciplines. The strong and negative effect holds 12 years and more after these gruesome war events took place, indicating how long the shadow of war can be for individuals.

Third, our paper also contributes to identifying and quantifying the effects of wartime military service. Following Angrist (1990), a prominent body of literature has used conscription-lotteries for service in World War II or the war in Vietnam to produce convincing estimates of the causal impact of veteran status on individual outcomes, such as future employment, earnings, disability status, mortality and crime (see e.g. review in Siminski, 2013). Most analyses are based on post-service comparisons between veterans and non-veterans in developed countries, where the exact service characteristics and experiences underlying observed post-service differences as well as impacts on families remain unidentified.[8] Our paper estimates the causal effect of a specific, prevalent, and intense wartime military service experience on a household outcome, based on variation in a developing country population.

Our findings have important implications for policy and practice, especially efforts targeting domestic violence and post-conflict reintegration.

Traditional policies aimed at reducing rates of domestic violence, for example, are often designed to empower women. Unfortunately, the effectiveness of such programs is mixed across contexts, and a few have even provoked perverse effects, as the male partners of women receiving support sometimes react violently to these interventions and their implications.[9] The findings of this study, by contrast, emphasize the need for and potential of paying more attention to perpetrators and the factors internal to them that lead to domestic violence. Working with affected and at-risk men may then

[7] In addition, measures of previous and current violent behavior need to rely on self-reporting, which invites systematic measurement error, and evidence from randomized controlled trials is obviously limited to simulating exposure to violence or maltreatment.

[8] A notable contribution from developing countries is the paper by Blattman and Annan (2010), which compares the labor market outcomes of former child soldiers vs non-child soldiers in Northern Uganda. Angrist and Johnson (2000), Heerwig and Conley (2013), and Negrusa and Negrusa (2014) compare family-level outcomes between US veterans and non-veterans, and document mixed and generally weak effects of deployment to war zones on marriage stability.

[9] In the US, for instance, state laws which require the police to arrest abusers when a domestic violence incident is reported, turned out to significantly increase domestic homicides (Iyengar, 2009).

offer an additional and fruitful route to reduce and prevent domestic violence.

Post-conflict reintegration programs often assume that former combatants pose a threat to political stability, focusing on short-term economic assistance to individuals. However, they often struggle to deliver large-scale, effective and persistent results (see e.g. review in Gilligan, Mvukiyehe and Samii, 2013). Our study exposes stability threats *at the family level* as a *long-term* legacy of war exposure. These insights can inform future assistance, tailored to and addressing the psychological conflict legacies carried by veterans.

The nature of uncovered mechanism suggests that the negative long-term impact on the veteran's and his families' well-being might be mitigated by innovative forms of behavioral training, such as cognitive behavioral therapy (CBT). Pilot studies of CBT programs have successfully reduced a host of criminal and violent behaviors among disadvantaged youth in Chicago (Heller et al., 2016) and among ex-combatants in Liberia (Blattman, Jamison and Sheridan, 2016).

The rest of the paper is organized as follows. Section 4.2 provides theoretical motivations for exposure to wartime sexual violence against women causing a lasting effect on the individual propensity to commit domestic violence. Section 4.3 describes the Angolan context and the survey data we collected and use in this paper. Section 4.4 presents the identification strategy, econometric specifications and assumptions of the empirical analysis. Section 4.5 presents the main results. Section 4.6 discusses the mechanisms underlying these results. Section 4.7 concludes and discusses policy implications.

4.2 Theoretical framework

4.2.1 From wartime sexual violence to post-war domestic violence

Mechanism 1: Reduction in man's bargaining power. Economists and criminologists emphasize the role of relative bargaining power within the household to explain domestic violence. In the collective perspective on household decision-making, each household member or spouse has their own preferences, and intra-household allocations are determined through a bargaining process.[10] An individual's welfare in case the household dissolves – their 'outside option' – determines their ability to influence household decisions – their 'bargaining power'.[11] In consequence, power proxy variables such as earnings ability, realized

[10]See e.g. Browning and Chiappori (1998), Chiappori (1988) and Blundell, Chiappori and Meghir (2005).

[11]See Chen and Woolley (2001) and Lundberg and Pollak (1993) for models of *within*-marriage outside options, and Lundberg and Pollak (1996) for an excellent general account of bargaining in

income or remarriage prospects, are assumed to furnish an individual more influence over intra-household decision-making and allocation.

Recent economic models directly incorporate domestic violence: individuals have preferences over consumption *and* violence, and bargain over the level of abuse and the allocation of consumption with their partner. It is essentially assumed that a partner may accept violence in exchange for intra-household transfers. A key prediction of these models is that *less* relative bargaining power for the man is associated with *less* domestic violence against the woman (e.g. Aizer, 2010). These results resonate with related criminology theories of 'female exposure reduction', which argue that better or more employment of the woman, i.e. a decrease in the man's relative bargaining power, is associated with less abuse (e.g. Dugan, Nagin and Rosenfeld, 1999).

A prominent literature in economics argues that military service in war zones may have negative long-term effects on human capital and labor market outcomes (e.g. Angrist, 1990; Siminski, 2013), hence effectively reducing a man's economic bargaining power. **Prediction:** *If past exposure to wartime sexual violence has lasting negative effects on the veteran's relative bargaining power, this mechanism predicts a negative effect on, i.e. less, domestic violence today.*

Mechanism 2: Change in traditional norms related to gender identity and relations. Various disciplines view domestic abuse as a response to specific cultural norms related to gender identity and relations. For instance, men may use domestic violence to coercively control their partner, assert or maintain manhood status, and extract resources outside the household (e.g. Macmillan and Gartner, 1999; Bloch and Rao, 2002; Dagirmanjian et al., 2016).

In sociological and economic models of collectives, it is well established that armed groups often manipulate existing beliefs (e.g. Costa and Kahn, 2003; Akerlof and Kranton, 2000), which specifically includes models of gender and masculinity. Gender-based analyses of armed conflict provide ample evidence how warlords deliberately re-interpret existing gender stereotypes (e.g. GIZ, 2009). **Prediction:** *If past exposure to wartime sexual violence has a lasting effect on the interpretation of norms of gender identity and relations, this mechanism predicts a positive effect on, i.e. more, domestic violence today.*

Mechanism 3: Creation of violent models. Social learning theory posits that individuals can also acquire new behaviors through modeling and reinforcement contingencies in the context of social interactions (Bandura, 1977).[12] Specifically, individuals may learn violent behavior when they are exposed to violent models

marriage.

[12]In the economics literature see e.g. Banerjee (1992) and Bikhchandani, Hirshleifer and Welch (1992)

(Bandura, 1973).[13] Such models of violence often arise within military collectives or are directly promoted by commanders (Littman and Paluck, 2015). With respect to gender-based violence, Wood (2012) establishes that wartime sexual violence by armed groups may be ordered, prohibited or tolerated by commanders. Social learning theory hence suggests that soldiers may 'learn' violence against women. In terms of the underlying processes, it distinguishes normative and non-normative forms of learning. Akers (2011), for instance, distinguishes "the direct association and interaction with others and their conforming or deviant behavior" (*behavioral/interactional*) and "the different patterns of norms and values to which an individual is exposed through association" (*normative*).

These arguments suggest that exposure to sexual violence against women during the war may create corresponding models of violence. Exposure may make soldiers align their behavior (*behavioral/interactional*) or lead them to internalize norms of violence against women (*normative*). **Prediction:** *If past exposure to wartime sexual violence instills lasting models of violence against women, this mechanism predicts a positive effect on, i.e. more, domestic violence today.*

While hitherto mostly ignored by economists, recent studies in psychology and public health offer explanations of domestic violence beyond the social environment and norms. These studies emphasize the role of personal and situational factors, which may, for instance, provoke domestic violence due to momentary failures in self-regulation (e.g. Finkel et al., 2009; Angelucci, 2008; Johnson, 2010; Card and Dahl, 2011). While most of the empirical evidence is correlational, many studies argue that pathologies can have their origins more than a decade ago, including exposure to traumatic experiences during military service (e.g. Dohrenwend et al., 2006). Based on these insights we hypothesize that a long-term, individual level causal impact on domestic abuse may operate via psychological channels.

Mechanism 4: Distress. Our first psychological mechanism considers the direct impact of the typically aversive nature of experiences of intense war violence. The psychology of violent acts suggests that for most individuals perpetrating or witnessing a violent act is associated with substantial psychological and physiological distress. World War II soldiers, for instance, reported not to have fired or deliberately misfired even when they were in the line of fire themselves (e.g. Grossman, 1996). Sexual violence in particular is considered a 'traumatic event', and can cause psychological struggles and negative emotional states, which may become chronic and lasting (Dekel, Mandl and Solomon, 2011). A burgeoning literature transcending disciplines report that these struggles may improve social relationships via 'post-traumatic growth' (PTG) (Tedeschi and Calhoun, 1996); for a review in economics see Bauer et al. (2016). Negative emotional states include guilt and shame, which exposure to sexual violence against civilian women during the war may very

[13] A well-studied example are models of intergenerational transmission of violence through this lens, which show that witnessing acts of violence at a young age, e.g. between parents, may result in violent behavior as an adult.

plausibly induce, and have been shown to be correlated with PTG (Dekel et al., 2016*a*). This logic would predict less domestic violence today among exposed veterans.

However, a distress-based mechanism may also provoke domestic violence, if exposure to violence causes or contributes to a psychological disorder, such as the widely discussed post-traumatic stress disorder (PTSD) (e.g. Dohrenwend et al., 2006; Cesur, Sabia and Tekin, 2013). Dekel, Mandl and Solomon (2011) show that among former prisoners of war, PTSD and PTG do share a number of jointly correlated factors. The average probability of PTSD following a traumatic event is less than 10%, but significantly higher for 'traumatic events of human design', and more than 30% of (all) PTSD cases become chronic (Dekel et al., 2016*b*). A host of studies show that PTSD among veterans is associated with more – rather than less – domestic violence (e.g. Sherman et al., 2006). This logic predicts more domestic violence today among veterans exposed to sexual violence. **Prediction:** *If past exposure to wartime sexual violence increases psychological distress in the long term, the effect on domestic violence today may be positive or negative.*

Mechanism 5: Conditioning into violent practice against women. An alternative psychological mechanism builds on psychological insights into the factors that deter many individuals from violent acts: they need to overcome an "outcome aversion" (Miller and Cushman, 2013) and an "action aversion" (Miller, Hannikainen and Cushman, 2014).[14] A large literature in social and military psychology shows that armed groups and warfare itself can effectively reduce these aversions (see review in Littman and Paluck (2015)).[15] Notably, this removal of psychological barriers may persist and lead to more violent behavior in the future; for a study in economics, see Rohlfs (2010).

We posit therefore that exposure to sexual violence may condition soldiers into 'violent practice against women', permanently reducing or removing psychological barriers to violence against intimate partners. We choose the term 'practice' to underline the idea that the nature of the violent act is then usually customary, and often automatic, rather than strategic. **Prediction:** *If past exposure to wartime sexual violence conditions soldiers into gender-based violent practice in the long run, this mechanism predicts a positive effect on, i.e. more, domestic violence today.*

[14]The two different types of aversion distinguish concerns related to (executing) a harmful action, and those related to its consequences, or outcomes, such as empathy with the victim's suffering.

[15]According to Littman and Paluck (2015) the dominant psychological processes of how this pattern comes into realization include "desensitization through repeated exposure to the act, shifting personal definitions of violence, disengaging from moral reasoning processes, and changing other attitudes or behaviors to reduce the experience of dissonance between personal standards and the act of violence."

4.2.2 Sexual violence by armed groups against civilians

Understanding why and when armed groups perpetrate (more) sexual violence in the first place, and which individuals are exposed to such events, is crucial for causal identification. Yet these questions are only poorly understood, and rigorous, statistical evidence is particularly rare (Cohen, 2013; Cohen and Nordås, 2014). A recent branch of the literature emphasizes variation in incidence based on the institutional make-up of the perpetrating organization (see e.g. Wood, 2015).

We build our argument on variation in sexual violence at the army level based on two specific sets of suggestive evidence on armed actors with high 'institutional capacity.' We define such a 'high-capacity actor' as either a sovereign state or a rebel organization that has the capacity to establish bounded monopolies of force and enforce 'state-like' public policies (locally), such as public good provision, taxation and compulsory military service.

The first set of studies documents that high-capacity actors fighting a civil war tend to be less likely to perpetrate sexual violence against civilians, compared to armed groups of different nature. For instance, Wood (2006) writes that "where insurgent groups depend on the provision of support (supplies, intelligence) from civilians and aspire to govern those civilians, they do not engage in sexual violence against those civilians if they have a reasonably effective command structure." Second, the dominant general perspective on wartime sexual violence – especially in the policy sphere – focuses on its role as a strategic 'weapon of war'. For example, UN Security Council Resolution 1820 states that sexual violence can be "used or commissioned as a tactic of war in order to deliberately target civilians or as a part of a widespread or systematic attack against civilian populations" (UN, 2008). While the recent literature emphasizes that wartime sexual violence against civilians may not necessarily be *ordered* (Cohen, Green and Wood, 2013), there is evidence from many contexts that mass rape may under certain circumstances indeed serve as a key instrument of powerful armed groups to terrorize civilians (e.g. Maedl, 2011). High capacity actors are no exception: A recent body of scholarship documents that also high-capacity actors sometimes order or tolerate sexual violence by its soldiers against civilians as a tool of torment, torture or terror (Wood, 2006; Cohen, Green and Wood, 2013). Examples include systematic, collective rape by state actors targeted at certain sub-populations (Green, 2006) and strategic sexual violence against detainees or individuals suspected of supporting an insurgency (Wood, 2006).

We therefore expect to observe temporal variation in the extent of sexual violence by a high-capacity actor. More specifically, we expect to see moderate baseline levels over time, and high levels only during confined episodes. We thus hypothesize that a soldier who serves in a time of one or more of these episodes will be more likely to be exposed to (more) wartime sexual violence than an otherwise identical soldier who serves at a different time.

4.3 The Angolan Civil War

Between 1975 and 2002, two highly organized and capable military factions fiercely battled for territorial control and their respective nation-building missions in the Angolan Civil War. These were the *Movimento Popular de Libertação de Angola (MPLA)* and the *União Nacional para a Independência Total de Angola (UNITA)*. Both organizations emerged as nationalist movements opposing the Portuguese colonial rule in the war of independence between 1961 and 1974, alongside the *Frente Nacional para a Libertação de Angola (FNLA)*. By the time independence was consolidated in 1975, inter-movement fighting had erupted over controlling the new nation. FNLA was to fold away soon, while MPLA, led by upper-class 'assimilados', seized control of the capital Luanda and became 'the government'. UNITA presented itself as the 'party of *all* Angolans' and initially seized control of much of the Southern and Eastern territories and became 'the rebels'.

Both parties managed to secure strong international allies and consistent access to natural resources. MPLA relied on assistance from Cuba, the Eastern bloc and oil revenues, while UNITA was backed by South Africa, the US and diamond trade (e.g. Guidolin and La Ferrara, 2007; Berman et al., 2016). This Cold War 'proxy-war' lasted from 1975 until 1991, and was characterized by large-scale front-line fighting, including the biggest conventional battle of post-WWII Africa in Kuito Canavale in 1987. The Cold War phase ended in 1991, when a ceasefire was agreed in the Bicesse Accords. After abortive elections in 1992, MPLA and UNITA returned to war, now without support by their Cold War allies. Extremely violent episodes and 'see-saw' battles ensued, only interrupted by a failed peace agreement in 1994. In February 2002, MPLA secured a clear and undisputed victory, when UNITA's leader Jonas Savimbi was assassinated in an ambush. Military operations ground to a halt abruptly and a Memorandum of Understanding was signed in April 2002, to be followed by rapid mass demobilization on both sides. Angola has since recorded more than a decade without mass violence.

We study outcomes in 2014, twelve years after the end of the war. While Angola by then experienced more than a decade of enormous macroeconomic growth, this was based almost exclusively on crude oil revenues and human development remained extremely low. State institutions and their reach remained extremely weak by 2014, preventing effective political and economic development (Soares de Oliveira, 2013). In comparison to countries of comparably low levels of human development, the number of NGO and foreign aid projects and their influence are also extremely small in Angola (Soares de Oliveira, 2011). In practice, this means that in most regions outside the capital, factors that may affect intra-household bargaining and the use of spousal violence have virtually not been affected by policy treatments. This technically benefits our study as it mitigates concerns that estimates of our link of interest may be conflated by the effect from the impact of post-war policy treatments.

4.3.1 Relevance

Angola's 27-year civil war offers a relevant setting for our research question. Immediately after independence, the nation experienced a very long and intense war, where territorial control was highly volatile.[16] On the other hand, the civil war was fought by the same two factions throughout, both with sufficient capacity to enforce compulsory military service for young men (see Chapter 2). Both sides engaged frequently and often systematically with the respective local populations under their control in a variety of constructive and destructive ways. These interactions between civilians and combatants ranged from peaceful policies such as the provision of public services by the armed group, protection from enemy violence and taxation schemes, to extensive violence against civilians, including mass killings and the sexual abuse of civilian women (e.g. Ziemke, 2008; Parsons, 2006). This configuration suggests that the population of former soldiers is a) large, b) likely to offer substantial variation in timing of service and c) likely to exhibit variation in military experiences, most importantly exposure to wartime sexual violence.

Huambo province. We focus the study on Angola's vast Central Highlands and its center, Huambo province, for two main reasons.[17] First, the Central highlands were at the center of the war. It was in Huambo City, Angola's second largest city, that UNITA proclaimed their own government on the same day MPLA declared independence from Portugal in Luanda, the 11th of November 1975. Most parts of the vast Central Highlands were occupied by the two movements at different times and changed hands multiple times, which makes it possible to compare directly the members, practices and dynamics of the rival organizations. Second, Huambo province is the most densely populated region in Angola, and ethnically homogeneous (Ovimbundu). While the political literature of the Angola Civil War argues that ethnicity was never at the root of the conflict (e.g. Pearce, 2012), this design also allows to rule out confounding individual factors related to ethnicity.[18]

[16] As Pearce (2012) notes, the end of the war in 2002 marked the first time since independence that the government had at least notional control of the entire Angolan territory, including large areas it had not held in a long time or ever. At certain points during the war, the government held as little as 20% of its nominal territory.

[17] Huambo province is roughly of the size of Switzerland; see Figure 2.2 in Chapter 2 for a map.

[18] An early Angola literature and cross-country studies classify the Angolan Civil War and its combatants in ethnic, religious and natural resource terms. In these accounts MPLA is related to the Mbundu ethnic group, urban *mestiços* with a Roman Catholic background and oil revenues, and UNITA to rural Congregationalist Ovimbundu and diamond trade (e.g. Marcum, 1989). Yet, the recent Angola literature contends that the central strategy on both sides was about securing the loyalty of all individuals living in regions under their controlled, seeking legitimacy for and demonstrate the state capacity of their own movement to lead a united Angolan nation, and portraying the rival movement as a muppet of foreign actors (Roque, 2015). From a civilian perspective, recent studies emphasize that non-combatants in contested territory would identify as "UNITA people" and "MPLA people" at different stages in their lives rather than based on regional, religious or ethno-linguistic background (Pearce, 2012). These insights suggest that, while ethnicity certainly had a role, it was not at the core of the state-level conflict. Being an ethnically homogenous region, the study site mitigates these potentially confounding concerns further.

4.3.2 Natural experiment in exposure to wartime sexual violence

We now discuss variation in sexual violence by armed actors during the Angolan war and argue that individuals were exogenously selected into different levels of sexual violence against women during their service.

Violence against civilians by MPLA

Historical accounts. We start by reviewing the historical evidence on temporal variation in sexual violence against civilians. The literature suggests generally moderate levels of sexual abuse by the two armed groups, which both were seeking to legitimize their state-building mission. Yet, there were two distinct episodes of systematic violence against civilians by MPLA, which plausibly included high levels of sexual violence. In contrast, not much evidence exists on variation in violence against civilians by UNITA.

The first episode of increased MPLA violence against civilians spans around five years in the late 1970s and early 1980s, when MPLA faced *internal* tensions and some factions mounted challenges to MPLA's leadership (Pawson, 2014). Starting in 1977, these developments involved street demonstrations, the breaking open of a prison, the hijacking of a state radio station, and the killing of several prominent party leaders. A repressive large-scale campaign of violence and terror against civilians ensued. While often considered a key turning point in Angolan history, the details of these events remain highly contested in Angola and discussing them is still a taboo for many Angolans (Pearce, 2015*b*).

Second, MPLA sharply increased violence against civilians during the final years of the war, after another peace agreement failed in 1998. This period is generally often referred to as "confusão," roughly meaning imbroglio, and involved all actors of the war. Notably, an overwhelming number of reports by human rights organization and observers noticed and lamented specifically a change in behavior by MPLA as a crucial factor feeding the confusão (e.g. Fonseca do Carmo et al., 2011). Reported atrocities by MPLA included widespread violence against civilians of various forms, as described in this HRW (2001, 2–3) report:

> The government's late 1999 and early 2000 offensives included a scorched earth policy, burning villages and killing civilians [...] In the central highlands, allegations of rape by government soldiers increased. Soldiers broke into houses and raped women, or raped women they encountered working in the fields. These occurrences were widespread near military camps. Rape was especially commonplace during batidas, house-to-house searches, when units arrived in an area, and ordered local people to collect food and non-food items for them and to help

transport looted goods. Those who refused to do so were often beaten and sometimes raped.

In addition to these two periods, the literature describes a brief third outburst of general violence against civilians following the presidential election in 1992. This episode, however, seems distinct in nature due to its political targeting and dominance of fatal violence. In this case, MPLA orchestrated a few weeks of extreme violence after UNITA rejected the results of the election. Hostilities started in Luanda and rapidly spread to other parts of the country. In the course of these events, MPLA forces reportedly massacred tens of thousands of individuals suspected to be a member or supporter of UNITA, including the so-called "Halloween Massacre" (Pearce, 2015*a*; Pereira, 1994).

Conflict event data. Unfortunately, systematic data of sexual violence during the Angolan war does not exist. Yet we can explore temporal variation in general "one-sided violence" against civilians quantitatively as coded in the conflict event data collected by Ziemke (2008).[19] Figure 4.1a plots all events of (any form of) violence against civilians over time. We observe the three expected types of variation: (i) across armies, (ii) over time (within armies) and (iii) how the differential between armies varies over time. The data confirm the historiographic evidence for *two* of the three distinct periods of increased violence against civilians by MPLA. First, post-electoral violence in 1992, and second, violence towards the end of the war, which we associate with sexual violence. Given the sensitivity of and mysteries around the events in the late 1970s ('Episode 1'), it seems almost by construction that these events do not show in the ACLED data.[20]

Survey data. Turning to our individual survey data, Figure 4.1b presents a local polynomial smooth plot of the self-reported exposure to sexual violence against a civilian woman (ever) over date of the respondent's military entry. The confidence bands are obviously large as the number of observations for aggregate measures such as 'year of entry' is limited. However, the visual intuition of the broader patterns is obvious. The graph suggests a local maximum in mean exposure for those MPLA soldiers (blue) who were more likely to serve a) in the late 1970s and b) at the very end of the war, where the running mean increases steeply. These maxima directly relate to Episodes 1 and 3 described above. We do *not* observe a local maximum for

[19]This dataset has been integrated into the cross-country dataset compiled by the Armed Conflict Location & Event Data Project (ACLED). It is a conflict event database for the Angolan War which includes data on (any) reported operations against civilians, coded by date and actor. One caveat is that the information is almost exclusively based on Portuguese and Portuguese-speaking media sources, which likely favored MPLA. This means that MPLA atrocities may be underreported.

[20]Based on the description of the the groups in the literature, we expect moderate average levels violence against civilians by either actor. Yet, even with these priors, the extremely low level of MPLA violence against civilians up to 1990 are not plausible. At least quantitatively, these patterns need thus be interpreted with caution.

MPLA soldiers who joined the military shortly before 1992. This pattern is consistent with an interpretation of the 1992 post-electoral violence as fundamentally different from the other two episodes of violence against civilians, in terms of the nature of violence. The brief period of strongly politically motivated mass killings by the MPLA government's forces appears not to have been accompanied by (reported) sexual violence.

Turning to UNITA soldiers (red) we observe two things. First, the overall mean exposure of UNITA soldiers does not appear to be noticeably different from that of MPLA soldiers (as expected due to the similar and high capacity nature of both). Second, the temporal variation within UNITA is very different compared to MPLA. These insights above suggest that the interaction of whether a soldier joined MPLA and whether he served in the late 1970s or at the very end of the war is an *informative* source of variation in exposure to sexual violence.

Exogenous selection into treatment

To understand the interaction just described and its exogenous origins, we now explore what factors determined which army a soldier joined and when.

Variation 1: Army. The Angola literature on war politics in the Central Highlands argues *against* strong 'selection' into either side based on conventional, individual factors associated with rebel recruitment or mobilization into armed groups (Weinstein, 2007; Berman and Laitin, 2008). Instead, the Angola literature documents that control of local territory shifted frequently and concludes that *"a person's first contact with any political formation was as likely to have been with UNITA as with the MPLA, and the processes that bound people to one or the other movement were similar on both sides."* (Pearce, 2012, 463). Pearce (2009, 4–5) adds that *"political identity was a matter of necessity rather than of conviction. It is for this reason that I use the word 'adherent' rather than 'supporter' when referring to the people who lived under the control of one or other movement during the war, since 'support' suggests a degree of voluntary affiliation which misrepresents the relationship."* As many other observers also emphasize, these accounts also show that the armed groups were able to exert strong control.

The survey data on recruitment date by army and region presented in Figure 2.4 are consistent with these findings. To produce these graphs, we divided the sample into five sub-samples based on the broad region where a soldier was recruited. These are the Center, North, West, East of Huambo province and a fifth category, into which all soldiers are pooled that were recruited outside Huambo province (about 6.5% of the sample).[21] For each region, the graphs reveal considerable variation over time in how likely it was (ex-post) to join a certain army. There is no region where individuals

[21] See also in the map presented in Figure 2.2.

were consistently more likely to join one army than the other throughout the war. Even though often both armies claimed control over pockets of territory in the same region at the same time, the negative correlation in the distribution over time between the two armies is striking (conditional on region).

Table B.1 presents results from a simple regression of the army indicator on the full set of family background characteristics.[22] The results confirm that no family background characteristic is a robust and systematic predictor of which army a soldier joined, with or without pre-service location fixed effects. We use classical standard errors as the least conservative choice. As more than 95% of all sampled veterans served in only one army, we conclude that the army indicator may be plausibly exogenously determined.

Variation 2: Date of entry. What determined the *date* of military entry? In state-controlled regions, military service was compulsory by law since 1976 (Junior, 2015). The law specified that military service was obligatory for men between 1 January of the year they turned 20 and 31 December of the year they turned 45.

The Angola literature points out that UNITA was a high-capacity organization which resembled a 'state in the state' (e.g. Roque, 2015). If UNITA also recruited 'like a state', the age and background distributions of UNITA soldiers should equally be consistent with conscription, as argued in Chapter 2. In particular, for soldiers of either side date of entry into the armed group and date of birth should be highly correlated. Figure 2.3b shows the distribution of age at entry and indeed reveals a strong concentration of mass entries in late teenage years, consistent with forced enlistment. The local regression plot in Figure 2.10a confirms that, as expected, there is an extremely close relationship between date of birth and date of military entry in both armies. This means that a) date of birth is a robust mean predictor of date of entry into an armed group and b) we should see a similar pattern in exposure to wartime sexual violence across date of birth as for date of entry (shifted by 20 years).

Identifying variation: Army x date of birth. We now consider the interaction of army and date of birth. Figure 4.1c displays plots of the exposure to sexual violence over date of birth (instead of date of entry now), and reveals that the patterns are indeed very similar. The difference it makes whether an individual joined MPLA (vs UNITA) depends on his year of birth. Years of birth around 19 years before Episodes 1 or 3 seem to have an MPLA soldier more likely to be exposed to sexual violence, compared to all others. The year-of-birth bins spanning five years each that correspond to Episodes 1 or 3 from above are bins III, VII and VIII of the eight total bins (I being the oldest cohorts and VIII being the youngest).

[22] Please note that numbers preceded by a 'B' indicate that the figures and tables are presented in the Appendix. For a related list of background characteristics see Blattman and Annan (2010); for a review of the mobilization literature see Blattman and Miguel (2010). See also Table 2.4, Table 2.5, Figure 2.6 and the detailed discussion in Chapter 2.

The patterns in these unconditional relationships suggest that the Angolan Civil War created an informative and valid source of variation in exposure to sexual violence by armed groups that is based on the interaction between the army the individual joined and his year of birth. To be precise, we expect that *soldiers that joined MPLA and were born into pooled birth cohort–bins III, VII and VIII were significantly more likely to be exposed to wartime sexual violence, compared to all other soldiers.*

Exclusion restriction. The identifying assumption is that the combination of joining UNITA plus being born into bins III, VII or VII) is uncorrelated with confounding factors. There is no obvious reason to believe that individuals 'selected' into this 'sexual violence sub-group' were systematically different in observable or unobservable background characteristics. The first source of variation, being born into bins III, VII or VII, is determined exogenously. The second source of variation, joining UNITA vs. joining MPLA, is also plausibly exogenous as suggested by the literature and explained above. Econometrically, we will exclude the *interaction* of the army and birth-cohort indicators in the 'second-stage' of IV-estimation while including their main effects. So, even if doubts remained about whether the army assignment was pre-determined, we argue that there is no immediate reason why the interaction with being born into bin III, VII or VII (as opposed to not) would have been pre-determined.[23]

The main concern for the validity of the strategy is whether the identifying interaction 'selected' individuals also into confounding other wartime (or post-war) variables. Especially, if sexual violence systematically coincided with other forms of violence against civilians and these are correlated with post-war domestic violence, we are not able to isolate the effect of exposure to sexual violence.

To address some of these concerns, we collected data on other forms of violence against civilians and enemy soldiers. We now discuss results from these variables graphically and provide related econometric evidence below. Most importantly, we collected data on basic exposure to two broad classes of violent operations against civilians: attacks involving fatal violence vs those of non-fatal violence.[24]

Figure 4.2 presents plots of local polynomial smooths of binary exposure to other forms of war violence over year of birth. For comparison, we include the sexual violence plot in Figure 4.2a. Starting with variation in MPLA soldiers (blue lines), these unconditional relationships reveal two main insights. First, violence not

[23]See e.g. Adhvaryu and Nyshadham (2015) for a logically similar argument based on the interaction of local rainfall and physical distance to health centers.

[24]Ethnographic studies of the Angolan conflict and the political and historical analyses described above, suggested the main examples of civilian massacres and looting. Ziemke (2008) reports strategic mass killings of civilians and argues that these were more likely after crucial battlefield losses. On the other hand, food is the most fundamental resource to sustain an armed group, and was, generally, often extremely scarce in the Angolan conflict (UNICEF, 1998). In our survey, 53% of respondents report that "often" or "very often" they were "that hungry or thirsty that you ate or drank things you would never have imagined to ever eat or drink."[25]

directed at civilians (Figure 4.2c) and witnessing civilian deaths have clearly different patterns (Figure 4.2b, left). Second, the temporal variation in exposure to massacres (Figure 4.2b, right) and especially looting (Figure 4.2a, right) appear somewhat similar to the variation in exposure to sexual violence among MPLA soldiers. Yet, turning to comparisons across armies, Figure 4.2a and Figure 4.2b reveal that among UNITA soldiers the temporal variation in looting and massacres is very different from that in exposure to sexual violence. This illustrates how the *interaction* of army and year of birth in exposure to sexual violence is completely different from all other experiences, which allows us to isolate variation exposure to sexual violence from other war experiences, most importantly looting and massacres.

This provides intuitive evidence that the combination of being MPLA and 'born into' a five year-bin that made it significantly more likely to serve in either period 1 or 3 from above, isolates exclusive variation in exposure to sexual violence. If this holds, this interaction is not only an informative but also a *valid* source of variation in exposure to sexual violence. In the next section, we also specify econometric tests related to the arguments for the validity of the instrument presented in this section.

4.4 Empirical framework

4.4.1 Research design and data

The population of this study are all living men who were ever part of an armed group during the Angolan War and reside in Huambo. The new survey data we use in this paper is based on a sample of 759 Angolan war veteran households from 34 different localities collected by the authors in the Study of Angolan Ex-Combatants (POEMA).[26] The quantitative component of POEMA was supported by an anthropological companion study, which included twelve months of ethnographic fieldwork preceding the survey (Spall, 2015). Qualitative findings have been used to assess the validity of our hypotheses, determine their relevance in the local context, refine the survey questionnaire design, interpret quantitative results and explore underlying mechanisms.

The survey dataset documents detailed information on war and military service experiences, pre-service background and post-war social, economic and political behaviors twelve years after the end of the war. Sampled veteran households had to complete up to three interviews. First, a private household-level interview, together with their (main) cohabitant partner, if they had one at the time of the survey. Second, a private individual interview with the veteran, which included extensive modules on pre-military service, military service and war experiences. Third,

[26]The main analysis of this article focuses on the sub-sample of 578 veterans who have a partner. As a robustness check, we test for possible selection into "not having a partner." We do not find a significant relationship between exposure to wartime sexual violence and having a partner in 2014 (see Table B.2).

partnered sampled veterans' (main) cohabitant partners were privately interviewed simultaneously, with a focus on intra-household and family outcomes, including domestic violence. Individual interviews with men were done by male enumerators, interviews with women by female enumerators.

The domestic violence measure is based on eight different acts of abuse, as suggested by the Demographic and Health Surveys Domestic Violence Module (DHS, 2016). Following the literature, our main outcome variable is a binary indicator (e.g. La Mattina, 2016), which equals one if the partner reports that she experienced at least one of these acts in the last 12 months. Our key explanatory variable is based on the veterans' wartime exposure to sexual violence against women. In our main specifications we use a binary measure, which equals one if the veteran reports having been exposed to at least one situation where a civilian woman was abused during the war.[27]

In Section B.1 we provide more detailed information on the sampling strategy, the different interviews, motives and nature of our key measures, alternatively constructed indices of exposure, potential issues of recall bias, and key summary statistics (Table B.3).

4.4.2 Econometric specifications

For the baseline estimates we specify linear models where the effect of interest β is estimated by OLS regression:[28]

$$Domestic_i = \beta Exposure_i + \gamma' X_i + \varepsilon_i \tag{4.4.1}$$

Here, *Domestic* is the indicator of contemporaneous individual engagement in collective public good production; *Exposure* denotes exposure to wartime sexual violence; X is a flexible vector of controls variables; ε is the error term.

The identifying assumption for a causal interpretation is that the exposure to wartime sexual violence and the error term are not correlated, conditional on the control variables we include (conditional independence assumption (CIA)). If the CIA holds, the regression derivative equals the average causal effect (ACE), conditional on this set of controls. If the CIA fails, the equality of the regression derivative and the ACE no longer holds.

We include the following sets of control variables across specifications. First, all specifications include location fixed effects to purge systematic variation across

[27] We prefer the binary indicator as it is less prone to measurement error, but also present results on frequency of exposure in Section 4.6.

[28] We prefer the baseline linear probability model because the statistical mechanics of clustering, fixed effects and instrumental variable are much more straightforward for linear models (e.g. Angrist and Pischke, 2008). Because of the dichotomous nature of our dependent variable, we also estimate non-linear models as a robustness check.

locales. Second, we add pre-military service – and therefore pre-treatment – family background characteristics and pre-treatment region fixed effects to control for pre-existing differences. Third, we add (potentially confounding) socioeconomic variables that have been linked to perpetrating partner violence and might simultaneously co-vary with wartime sexual violence. Fourth, we sequentially add potential wartime confounders, such as experiences of violence, again bearing in mind that these may not be determined exogenously themselves.

Even after carefully controlling for these factors, we may measure exposure to wartime sexual violence with systematic error or may not be able to control for all individual factors that are associated with participation in both wartime sexual violence and post-war domestic abuse. Either would result in spurious estimates, and we rely on IV estimates to mitigate these concerns.

IV. The instrumental-variables (IV) strategy is based on linear models, as in Equation (1), where the effect of interest β is estimated by IV/2SLS regression.[29] To test the relevance of the instrument(s), we use OLS to estimate δ, as specified in the following 'first-stage' equation:

$$Exposure_i = \delta MPLA_i \times YOB - Bin_i + \lambda' X_i + \nu_i \tag{4.4.2}$$

Here, *Exposure* is experience with sexual violence by armed groups; X is the full vector of control variables, including the main effects of *MPLA* and *YOB-Bin*; ν is the error term.

YOB-Bin is a categorical variable with eight categories, defined by the cut-offs defined in the previous section. Unless explicitly stated, the fifth bin is the omitted category.[30] As a single instrument is favorable in terms of bias as it is approximately median-unbiased (Hahn and Hausman, 2003; Angrist and Pischke, 2008), we choose our single best instrument for our main specifications, where we interact the *MPLA* dummy with a binary indicator *YOB-Bin378*. *YOB-Bin378* equals one if an individual year of birth falls into pooled birth cohort 3,7 or 8. We then produce IV/2SLS estimates of Equation (4.1) using this 'first-stage' equation.

Identification. The identifying assumption of the IV-strategy is that the distribution of the instrument, projected onto included controls, is uncorrelated with

[29] In the presence of heteroskedasticity or intra-cluster correlation, the IV coefficient estimates are consistent (yet inefficient), but their standard errors and the usual forms of the diagnostic tests are not. To achieve asymptotically correct inference, in our baseline specifications are robust standard errors, corrected for clustering at the pre-service and post-service location levels, as in the OLS regression.

[30] This bin was chosen as veterans born in these years were most likely to serve in the late 1980s which were characterized by large-scale conventional battlefield war and likely less interaction with civilians.

the error term in Equation (4.1). One way to build confidence in the validity of the instrument is to test if it is correlated with observable pre-service background characteristics using the following specification:

$$MPLA_i \times YOB - Bin378_i = \pi' K_i + \nu_i \tag{4.4.3}$$

Here, *MPLA* × *YOB-Bin378* is the interaction the *MPLA* dummy with a binary indicator *YOB-Bin378*; K is the vector of twelve pre-service background characteristics; ν is the error term. We expect that no component of π is statistically significant from zero.

To explicitly explore whether the 'selection' into MPLA based on family background differs in any systematic way across bins, we also estimate Equation (4.3) with *MPLA* as the dependent variable, for the *YOB-Bin378* observations and those from other bins, i.e. not in *YOB-Bin378*, separately. Below, we will also conduct further tests related to the exclusion restriction by estimating Equation (4.2) using other war and post-war outcomes as the dependent variable.

4.5 Main results

4.5.1 OLS

Table 4.1 reports results from linear regression. Column 1 presents the most parsimonious form of our effect of interest, relying only on local variation. We observe a positive and statistically significant impact of exposure to wartime sexual violence on post-war domestic violence.

Columns 2 to 7 show that the positive relationship is robust in terms of magnitude and statistical significance, when we include village fixed effects, pre-service background variables, a series of other control variables, with two-way clustered standard errors.[31] Notably, the general indicator of exposure to operations against civilians included in column 5 does not affect the effect of interest and is not statistically significant. The same is true for related indicators of exposure to other forms of intense war violence, which have been linked to other post-war outcomes

[31]For the main specifications we follow Cameron, Gelbach and Miller (2011) to estimate the standard error. Observations may not be independent within two sets of locations: where individuals reside, and where they were recruited from. We thus estimate robust standard errors clustered by the primary sampling unit (*bairro*) and municipality of recruitment. 'Few cluster' issues and alternative techniques to estimate the standard error are discussed later in this section and in more detail in B.2. As also suggested by Cameron, Gelbach and Miller (2011), we rely on conservative inference and ignore stratification and survey weights (these less conservative results are very similar and available upon request).

(e.g. Blattman, 2009; Bellows and Miguel, 2009). These include witnessing fatal violence against enemy soldiers, fellow soldiers and civilians.

Further robustness. In B.2, we show that the main result is robust to classical, Huber-White, Moulton and wild cluster bootstrapped standard errors (Table B.4) and non-linear model specifications (Table B.5). Nonetheless, the effect could still be driven by correlations with unobserved confounding variables or systematic measurement error. To mitigate these concerns we rely on IV-estimation, which must be balanced, however, against a loss of efficiency vis-à-vis OLS. If exposure to sexual violence by armed groups is not correlated with the error term, the asymptotic variance of the IV estimator is always larger than that of the OLS estimator. We discuss the relevance of IV estimation, and the informativeness and validity of the instrument below.

4.5.2 Importance, relevance and validity of IV

The two primary endogeneity concerns are that the extent of exposure to sexual violence may be correlated with confounding a) individual background characteristics or b) other wartime experiences. For instance, those exposed to more sexual violence may be those with worse education or much more involved in battlefield fighting than others. If either has an impact on post-war abusive behavior, then the simple OLS estimate will be biased.

Table 4.2 explores individual traits associated with exposure to sexual violence during the war. Based on our survey data, it compares raw means between the two subs-samples defined by the binary exposure to sexual violence-indicator. The top panel suggests that the two sub-samples do not differ much in terms of characteristics related to the military, including army joined, role, rank or age at entry. Exposed soldiers report slightly higher levels of self-assessed unit mortality rates and were more often part of a unit operating mostly in combat areas. The bottom panel presents summary statistics on the five-year-bins of pooled birth cohorts and shows that veterans belonging to the exposed sub-sample are not distributed differently than non-exposed counterparts across bins.

By contrast, the middle panel suggests that the exposed sub-sample self-reports systematically different individual pre-service traits than then non-exposed. A veteran exposed to sexual violence is more likely to report having been a student and having had any schooling at the time of recruitment. Based on subjective health assessments, self-reports suggest that exposed veterans were significantly stronger, physically and mentally.[32] We observe no differences in self-assessed overall health and slightly less reports of having had formal training among the exposed.

[32] Respondents were asked to compare the composition to that of other individuals of about their age at the time of recruitment.

While this descriptive evidence needs be interpreted with caution, it suggests two central implications. Military-related traits and the year-of-birth distribution do not differ substantially across the two sub-samples, while individual self-reported and –assessed traits do. The latter deepens concerns that simple OLS results may be spurious.

Relevance. Table 4.3 presents first stage results. As expected, being with MPLA and born in bins 3, 7 or 8 predicts a substantial increase in exposure to sexual violence, conditional on MPLA and bin-of-year main effects (columns 1 to 4). Columns 5,6 and 7 shows the results for the binary instrument. Columns 2 to 7 demonstrate that both the additive as well as the single instrument are strongly relevant, with and without the inclusion of (basic) pre-/post-treatment controls, and in all specifications the F-statistic exceeds the critical value of 10.

Validity. Table 4.4 compares raw means in individual characteristics between the sub-samples now defined by the binary instrument.[33] As for the actual exposure to sexual violence, military-related traits do not differ substantially across the two sub-samples, and now, by contrast, individual self-reported and –assessed traits do not differ systematically, either. Table B.6, columns 1 and 2, confirm that no family background characteristic predicts the value of the instrument variable, with and without pre-service location fixed effects. Columns 3 to 6 present results from a regression of the army dummy on family background characteristics for YOB-Bin378 soldiers and others separately. With and without pre-service location fixed effects, we find no substantial differences in 'selection' for these specific bins. The results displayed in Table B.7 confirm that the instrument is not correlated with other war experiences. In particular, the IV is not a relevant source of variation in exposure to looting and massacres, as expected based on the graphic illustration in Figure 4.2. Finally, the results displayed in Table B.8 confirm that the IV does not predict variation in any potentially relevant post-war outcome.

4.5.3 IV

IV-estimates and the corresponding OLS-result from the same specifications are displayed in Table 4.5. The IV estimates (columns 2 and 3) confirm the positive effect of exposure to wartime sexual violence on domestic violence 12 years after the end of the war found by OLS (column 1). IV estimation returns point estimates around .3, which is considerably larger in magnitude than the OLS coefficients. As IV estimate corresponds to 68% of the sample mean of domestic violence, the IV estimates suggest very meaningful effects.

We expect gains in efficiency when we use the three interaction terms as additive

[33] We omit the army and year-of-birth indicators on which the instrument is built.

instruments instead of collapsing them into a single instrument. As reported in column 3, this indeed improves efficiency, but only very modestly. Similarly, the coefficient only changes marginally. As an additional robustness check, column 4 presents IV-estimates from a strategy that uses mean exposure in armed group-YOB-Bin cells as an instrument for individual exposure. For example, exposure of a UNITA veteran born in YOB-Bin 4 is instrumented with the simple average exposure over all UNITA veterans born in YOB-Bin 4. Again the estimate is qualitatively and quantitatively similar, with a statistically significant coefficient of .28. The stability of the IV estimate across alternatives further increases confidence in its reliability.

4.6 Mechanisms

Based on the theoretical arguments laid out in Section 4.2, a positive link between exposure to wartime sexual violence and post-war domestic violence is consistent with four mechanisms of impact: a change in traditional norms related to gender identity and relations (mechanism 2); learning of violent models (mechanism 3), which could be behavioral/interactional or normative in nature; distress (mechanism 4); and conditioning into violent practice against women (mechanism 5), based on a permanent reduction of a psychological barrier. To assess the explanatory power of these alternatives, we now present and discuss evidence from different aspects of domestic violence and conflict.[34]

Even though the sign of the main effect is not consistent with the predictions by mechanism 1 (via economic bargaining power), the effect may actually be present, too, but dominated by other mechanisms at work. The result reported in B.2.2 do not lend any support to the existence of such a mechanism.

Shift in gender norms and social learning based on violent models. Table 4.6 reports effects on behavior that is closely related to norms of gender and masculinity. Columns 1 to 4 document a very weak and inconclusive impact on domineering: exposed veterans are not significantly more likely to try to control their partners' relationships with girl friends or family members. We next investigate whether the exposure to sexual violence affects how much veterans try to live up to (related) norms of masculinity. In the ethnographic companion project, Spall (2015) emphasizes the role of fathering and fatherhood in Angolan war veterans' pursuit of living up to masculine ideals. We therefore test whether exposed veterans are more supportive of or have better overall relationships with their children, as reported by their spouse. The results presented in columns 5 to 8 provide evidence against such an effect. Partners of exposed veterans are not systematically more likely to report strong support of their children by the veteran or particularly good relationships between the veteran and his children. The impacts are very small in magnitude and,

[34] All outcomes in Table 4.7 and Table 4.6 are reported by the veteran's partner in a private interview.

if anything, slightly negative for support of children. These results reject a dominant role of a mechanism rooted in shifts in traditional beliefs about gender and masculinity.

Looking at physical and sexual components of domestic violence separately, Table 4.7 reveals that the positive main effect is exclusively driven by *non-sexual* acts (columns 1-2). Even considering that only about 12% of partners report having experienced sexual violence, the effect on sexual abuse is extremely small in magnitude and not statistically significant (column 3-4). Partners of exposed veterans are more likely to report frequent fights between partners (columns 7 and 8) and that they are often afraid of their husbands (columns 9 and 10).[35] Indirect evidence from non-physical outcomes adds further evidence on the violent nature of the impact. Partners of exposed veterans are more likely to report a) frequent fights between partners (columns 7 and 8) and b) that they are often afraid of her husband (columns 9 and 10).[36] These results underscore the violent and physical intensity of the abuse. The strong effect on physical violence and absence of variation in sexual violence provide evidence in favor of a psychological mechanism rather than imitating or normative behavior against women.

To test this conjecture further, we investigate the degree to which our main effect is accompanied by impacts on non-gender based violence. If soldiers were prone to learn behavior from violent models, we may see similar effects for other violent behaviors and attitudes. Specifically, we regress measures of attitudes toward political violence and engaging in work or activities that often imply violence, using the same parsimonious specification as before. The results reported in Table 4.8 suggest that exposed veterans are not more likely to agree with the use of violence by protesters in political demonstrations (column 1). The coefficient is very small and not statistically significant. By contrast, exposed veterans are significantly more likely to agree that police are justified to respond violently to protests (column 2). In addition, we find a negative effect on staying at a (hypothetical) protest that turns violent (column 7).[37] Finally, exposed veterans are not more likely to have engaged in illicit activity or work as a security guard in the last four weeks. Again, the coefficient is very small and not statistically significant. In sum, we do not find evidence that the strong impact on gender-based violence is accompanied by strong effects on attitudes or activities related to various forms of political or street violence. While the results are only suggestive, they are consistent with the psychological typology of the male 'family only batterer' (Holtzworth-Munroe and Stuart, 1994), yet not necessarily limited to it.

As a final exercise, Table 4.9 reports insights into the role of eight specific factors for the effect of exposure, which could be associated with learning and unlearning of

[35]Questions about severe injuries suffered from physical violence by their partner were based on the standard DHS module.

[36]We report the overall means of each (binary) outcome variable, to show that these strong effects are of about the same order of magnitude as our main effect.

[37]Question: "Imagine that you are at a protest and it turns violent, would you stay or leave?".

violence: tenure with the armed group, armed group (MPLA or UNITA), time elapsed since leaving the group, formal demobilization, formal DDR program, ties with members from armed group units, the village-level share of exposed veterans (of sampled veterans in the village) and the village-level share of veterans committing domestic violence (of sampled veterans in the village). The only factor that significantly affects the magnitude of our effect of interest is the village-level share of exposed veterans. A higher share may contribute to sustain and reinforce imitating or normative behavior. Yet, residing in village with more exposed veterans diminishes – rather than increases – the effect of interest.

In summary, these findings provide further evidence against learned or unlearned behavior and demonstrate that the effect of interest is persistent; it does not get weaker over time and it is not mitigated by demobilization or reintegration treatments.

Distress and gender-based violent practice. A psychological mechanism may operate via long-term a stress disorder (mechanism 4) or violent practice against women (mechanism 5).

In essence, either mechanism may reflect a lack of self-control in situations of discussions and decision-making, which we denote as 'deliberations' henceforth, as well as situations of escalating disputes between partners.

To better understand the extent of deliberation among veteran couples, we first look at three potentially contentious topics, for which likely no strong gender norm exists: how to spend/save income generated by the partner, how many children to have, and how conflicts with individuals that are not members of the household are resolved. The binary variables reported in columns 1-3 of Table 4.10 were coded as one if the couple reports that both the partner and the veteran are involved in deliberation. Two findings are apparent. First, deliberative mechanisms exist for the majority of households, exceeding 80% for each outcome. Second, households of exposed veterans are, if anything, more likely to deliberate. We find no significant difference with respect to partner's income and reproduction, and an economically small but strongly significant effect for conflict resolution outside the household.

Such discussions may escalate, however, into heated disputes, which in turn can provide a trigger for violent acts as in 'in the heat of the moment' arguments. As reported by the partner, exposure to wartime sexual violence is associated with significantly more disputes in the last twelve months (column 4).[38] Notably, among couples with disputes, partners of exposed veterans are not more likely to report that the veteran is more likely to start the dispute (column 5) or be more aggressive in disputes (column 6) than themselves.

Finally, the results displayed in columns 7 and 8 reveal a strong heterogeneity of

[38]Question: "In the last 12 months, how often did you and your spouse have major disputes?". Answer options: "Very often", "Often", "From time to time', "Rarely", "Never".

our main effect between couples with and without disputes (which is endogenous to exposure, as we just noted). While the effect is strong and highly significant for couples with disputes, it vanishes for couples without disputes. In combination with the first insight, i.e. that dispute incidence is more likely for exposed veterans, this suggests that the mechanism of impact does involve a lack of self-control.

Table 4.11 reports results on the effects on specific distress-related outcomes. We use the Rotterdam Symptom Checklist (RSCL), adapted to and validated for the Angolan context (McIntyre and Gameiro, 2013; Maia et al., 2011). The RSCL integrates direct assessments of mental health and the corresponding physical symptoms. We find no evidence for higher levels of contemporary distress among exposed veterans. Instead, we find that exposed veterans are slightly *less* likely to suffer from psychological distress (column 1) and show related physical symptoms (column 2). The negative effect on physical symptoms is statistically significant. The results in columns 3 and 4 suggest that exposed veterans are 5.4 percentage points more likely to report that they consume alcohol, statistically significant at the 90% confidence level, but less likely to smoke cigarettes, statistically significant at the 99% confidence level. Based on the overall inconclusive and rather negative direct effects on variables related to distress, we rule out a dominant role for psychological distress in shaping domestic abuse. In addition, these results are consistent with the weak impacts on bargaining power, which sometimes reflects psychological well-being and the associated physical status.

Intensive margin. While we believe that the binary indicator to exposure to wartime sexual violence is more reliable, we now discuss effects of the extent of exposure as a final exercise. Among veterans who report having been exposed at least once, 85% report total numbers of situations of five or less while 2% state numbers of twenty or more. The questionnaire design was based on psychological survey instruments, but self-reported recall data on the number of different situations may be an issue, of course, and qualifications are necessary. Veterans may be likely to not remember the exact number or feel uncomfortable revealing 'a large number', which would probably lead to underreporting. Overall, the reported absolute numbers may seem 'small' or 'too small', but they are qualitatively consistent with the sexual violence and Angola literatures cited above, which predict and describe sexual violence by armed groups in the Angolan conflict as rare – rather than frequent – events.

Table 4.12 reveals that the binary indicator masks important non-linearities in the effects of exposure. For the regression analysis we split the group of exposed soldiers into "1 to 4 situations" and "5 or more situations" sub-groups. The reference group are non-exposed soldiers. The results in column 1 suggest that the positive impact of exposure is driven by the less exposed, while the effect completely disappears for the more exposed. This pattern suggests that above a certain threshold the violent practice effect is dominated by a mechanism that works in the opposite direction, i.e. one that makes domestic violence *less* likely. In column 2 we effectively push mean

exposure in the high exposure group slightly higher by setting the threshold at six situations or higher.[39] While only suggestive, based on very small sub-sample analysis and not statistically significant, we observe that in this specification the high-exposure now is actually *less* likely to perpetrate domestic violence than the non-exposed, while the positive coefficient on the less exposed stay broadly constant. Columns 3 and 4 demonstrate that these results hold even when we drop extreme values of more than 50 situations.

To test whether the distribution of psychological distress reflects these competing forces, columns 5 to 8 report results on psychological distress. Across specifications, we find that belonging to the high-exposure group is associated with *increases* of average levels of the psychological distress index and related physical symptoms. These results suggest two things. First, they provide further support for the claim that the exposure to wartime sexual violence and perpetrating post-war domestic violence are linked by psychological processes. Second, the main effect is non-linear. Above a certain threshold, the dominant mechanism based on violent practice against women vanishes and 'high exposure' is associated with less domestic abuse (compared to no exposure), plausibly due to high levels of psychological distress.

4.7 Concluding remarks

This paper studies the long-term origins of domestic violence at the individual level. Using primary survey data and a natural experiment from Angola, we demonstrate that exposure to wartime sexual violence by armed groups against civilian women significantly increases a former soldier's propensity to commit domestic violence more than a decade after the end of the war. We argue that this effect is caused by being conditioned into gender-based violent practice, and that the effect is psychological and persistent in nature.

For the average effect, we find no support for alternative theories based on a reduction in economic bargaining power, living up to norms of gender and masculinity, the creation of violent models, or distress. In addition, we do not find evidence that the strong impact on gender-based violence is accompanied by meaningful effects on non-gender based violence. However, we find suggestive evidence that above a certain threshold of exposure intensity the main mechanism vanishes, and is dominated by high psychological distress. These results challenge dominant interpretations of domestic violence as instrumental behavior.

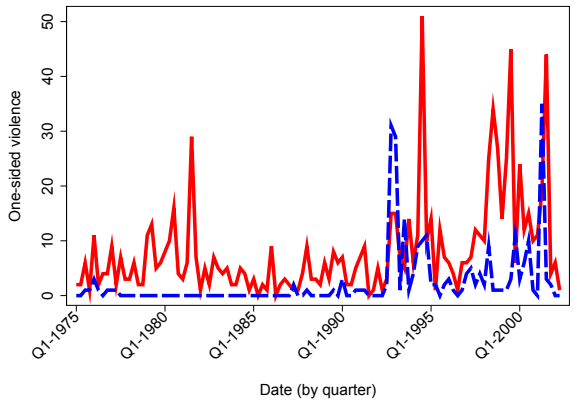
The findings have important implications for post-conflict assistance and domestic violence policy. Efforts to reintegrate former combatants have been shaped by fears that these threaten community stability and cooperation. Yet this study demonstrates that exposure to war violence may (also) threaten stability and cooperation at the

[39] We do not define higher thresholds because of the already very small sample size of the high exposure group.

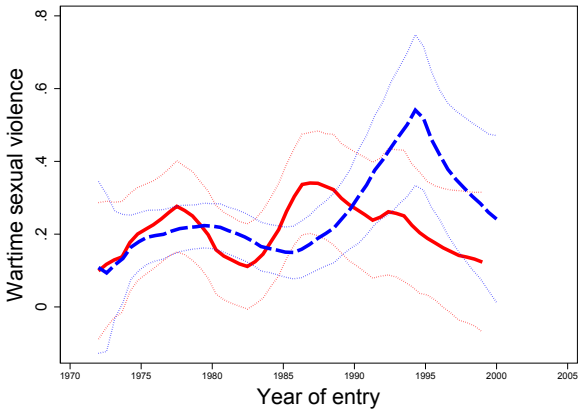
family level, including health risks for intimate partners. Policies aimed to reduce rates of domestic violence usually operate via increases in female opportunity, but have sometimes provoked unintended and perverse effects as men in treated households and communities may react with more violence. Our study emphasizes the need and potential of paying more attention to *male*, intrapersonal origins of domestic violence, which have only rarely been explicitly identified or targeted.

Figure 4.1: First-stage mechanism

(a) Event data: (Any) violence against civilians



(b) Wartime sexual violence against women over date of entry



(c) Wartime sexual violence against women over date of birth

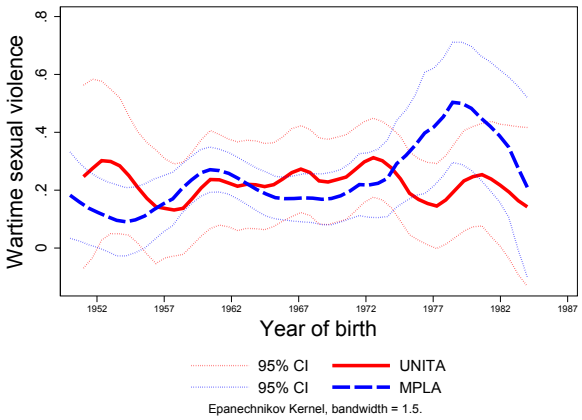
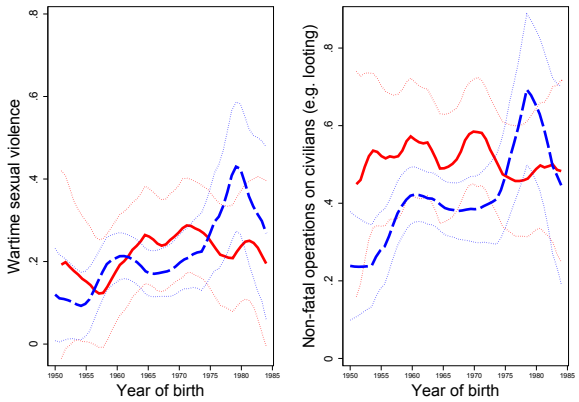
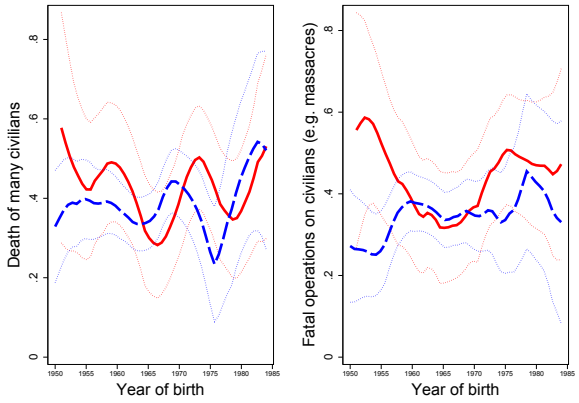


Figure 4.2: Exclusion restriction: different forms of war violence

(a) Non-fatal violence against civilians



(b) Fatal violence against civilians



(c) Fatal violence against combatants

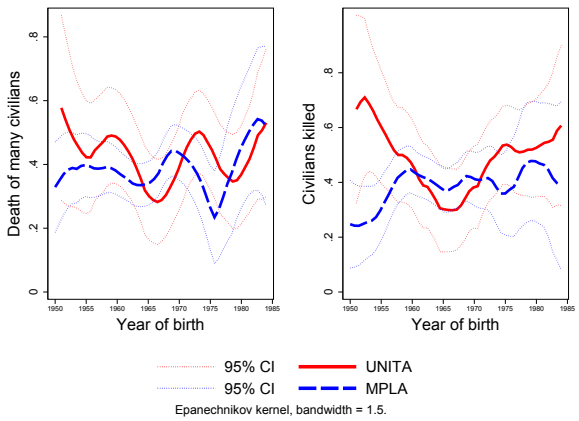


Table 4.1: Domestic violence and exposure to wartime sexual violence (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Wartime sexual violence	0.119**	0.130***	0.130***	0.113**	0.132***	0.126***	0.121**
	(0.054)	(0.047)	(0.047)	(0.055)	(0.050)	(0.044)	(0.048)
MPLA (main)		−0.009	−0.008	−0.006	−0.009	−0.010	−0.007
		(0.047)	(0.071)	(0.049)	(0.047)	(0.048)	(0.048)
Length of service			0.000				
			(0.005)				
Civilians targetted				0.040			
				(0.037)			
Fatal violence...							
..fellow soldiers					−0.012		
					(0.042)		
..enemy soldiers						0.016	
						(0.039)	
..civilians							0.041
							(0.039)
Post-Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	No	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578
R^2	0.09	0.18	0.18	0.19	0.18	0.18	0.19

Note: *Wartime sexual violence* equals one if respondent reports having experienced at least once a situation where a civilian woman was sexually abused, during wartime. *Civilians targetted* equals one if respondent reports having experienced at least once an operation that targetted civilians or their property and was not a massacre, such as looting. *Fatal violence...fellow soldiers* equals one if respondent reports having experienced at least once a situation where many or most fellow soldiers lost their life, during wartime. *Fatal violence...enemy soldiers* equals one if respondent reports having experienced at least once a situation where many or most enemy soldiers lost their life, during wartime. *Fatal violence...civilians* equals one if respondent reports having experienced at least once a situation where many or most civilians lost their life, during wartime. *MPLA* equals one if main army was MPLA (zero means UNITA). *Length of service* is wartime military service in years. *Post-controls* include these variables: respondent's age, age squared, years of schooling, and a binary measure indicating whether the woman earns more cash income. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.2: Who gets exposed to wartime sexual violence?

	Sample			Difference	
		Exp. to sexual violence			
	Full	Yes	No	Diff	p
Military					
MPLA (main)	0.70	0.66	0.71	−0.05	0.24
Unit: combat	0.81	0.86	0.79	0.07*	0.06
Unit: survivors	59.81	49.92	62.35	−12.43***	0.00
Role: Infant	0.43	0.44	0.43	0.02	0.71
Low rank	0.47	0.45	0.48	−0.03	0.56
Age at (first) entry	19.57	19.17	19.67	−0.50	0.39
Individual					
Was student	0.23	0.35	0.20	0.15***	0.00
Had any schooling	0.62	0.71	0.60	0.11**	0.02
Had any training	0.06	0.03	0.07	−0.04*	0.07
Self: health	5.12	5.13	5.12	0.00	0.99
Self: strength	6.00	6.50	5.87	0.63***	0.01
Self: mental	6.24	7.07	6.03	1.04***	0.00
Pooled birth cohorts					
Bin1: YOB $\leq$ 1950	0.12	0.11	0.12	−0.01	0.83
Bin2: 1950 $>$ YOB $\leq$ 1955	0.09	0.06	0.09	−0.04	0.14
Bin3: 1955 $>$ YOB $\leq$ 1960	0.16	0.13	0.17	−0.05	0.17
Bin4: 1960 $>$ YOB $\leq$ 1965	0.23	0.24	0.23	0.02	0.66
Bin5: 1965 $>$ YOB $\leq$ 1970	0.18	0.19	0.18	0.01	0.72
Bin6: 1970 $>$ YOB $\leq$ 1975	0.12	0.14	0.11	0.03	0.41
Bin7: 1970 $>$ YOB $\leq$ 1980	0.06	0.08	0.05	0.02	0.29
Bin8: YOB $>$1980	0.04	0.06	0.04	0.01	0.45

Note: *MPLA* equals one if main armed group was MPLA (zero means UNITA). *Unit: combat* equals one if unit(s) mostly operated in combat areas. *Role: infant* equals one if was infant (low role). *Rank: private* equals one if was private (low rank). *Age at (first) entry* is age when joined armed group in years. *Was student* equals one if was student at time of recruitment. *Had any schooling* equals one if had any schooling at time of recruitment. *Had any training* equals one if had any training at time of recruitment. *Self: health* is a subjective assessment of overall health compared to other recruits of about same age at time of recruitment. *Self: strength* is a subjective assessment of physical strength compared to other recruits of about same age at time of recruitment. *Self: mental* is a subjective assessment of mental strength compared to other recruits of about same age at time of recruitment. *Year of birth bins* are pooled birth cohort indicators and equal one if was born in indicated range. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.3: First-stage

	Exposure to wartime sexual violence						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MPLA x YOB-Bin 3/7/8					0.221***	0.219***	0.221***
					(0.057)	(0.051)	(0.053)
MPLA x YOB-Bin 1	0.088						
	(0.097)						
MPLA x YOB-Bin 2	−0.164						
	(0.151)						
MPLA x YOB-Bin 3	0.133***	0.136***	0.100***	0.106**			
	(0.039)	(0.038)	(0.033)	(0.043)			
MPLA x YOB-Bin 4	0.055						
	(0.078)						
MPLA x YOB-Bin 6	−0.041						
	(0.083)						
MPLA x YOB-Bin 7	0.423***	0.421***	0.459***	0.472***			
	(0.143)	(0.138)	(0.127)	(0.123)			
MPLA x YOB-Bin 8	0.213***	0.211***	0.223***	0.191***			
	(0.073)	(0.055)	(0.057)	(0.059)			
Pre-controls	No	No	No	Yes	No	No	Yes
Post-controls	No	No	Yes	Yes	No	Yes	Yes
Pre-Location	No	No	Yes	Yes	No	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578
R^2	0.10	0.10	0.14	0.16	0.09	0.13	0.15
F(MPLA x COB = 0)	9.89	11.97	41.84	31.77	15.19	18.70	17.58

Note: *YOB-Bin x* denotes pooled year of birth-cohorts (reference bin: $1965 > \text{YOB} \leq 1970$) *MPLA x Bin3/7/8* denotes the interaction of being MPLA and being born pooled year of birth-cohort 3,7 or 8. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.4: IV check 1: Are individual traits correlated with binary instrument?

	Sample			Difference	
		Excl. var. equals one			
	Full	Yes	No	Diff	*p*
Military					
Unit: combat	0.81	0.82	0.80	0.01	0.82
Unit: survivors (0-100)	59.81	58.72	59.95	−1.22	0.79
Role: infant	0.43	0.45	0.43	0.02	0.75
Rank: private	0.47	0.50	0.47	0.03	0.62
Individual					
Was student	0.23	0.22	0.23	−0.01	0.88
Had any schooling	0.62	0.67	0.62	0.05	0.36
Had any training	0.06	0.08	0.06	0.02	0.45
Self: health (1-10)	5.12	5.30	5.10	0.20	0.50
Self: strength (1-10)	6.00	5.94	6.32	−0.38	0.40
Self: mental (1-10)	6.24	6.43	6.22	0.22	0.49

Note: *Unit:combat* equals one if unit(s) mostly operated in combat areas. *Role:infant* equals one if was infant (low role). *Rank:private* equals one if was private (low rank). *Age at (first) entry* is age when joined armed group in years. *Was student* equals one if was student at time of recruitment. *Had any schooling* equals one if had any schooling at time of recruitment. *Had any traininging* equals one if had any training at time of recruitment. *Self: health* is a subjective assessment of overall health compared to other recruits of about same age at time of recruitment. *Self: strength* is a subjective assessment of physical strength compared to other recruits of about same age at time of recruitment. *Self: mental* is a subjective assessment of mental strength compared to other recruits of about same age at time of recruitment. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.5: Domestic violence and exposure to wartime sexual violence (IV)

	OLS	IV		OLS	IV	
	(1)	(2) 3+7+8	(3) Bin means	(4)	(5) 3+7+8	(6) Bin means
Wartime sexual violence	0.110**	0.334*	0.298*	0.121***	0.340**	0.319*
	(0.061)	(0.188)	(0.157)	(0.047)	(0.180)	(0.170)
Add. controls	No	No	No	Yes	Yes	Yes
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578
R^2	0.12	0.05	0.05	0.20	0.09	0.10
F-stat(IV)		14.60	24.48		28.80	24.99

Note: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.6: Domineering and masculinity

	Domineering partner		Role as father	
	(1) Family	(2) Friends	(3) Support	(4) Relationship
Wartime sexual violence	−0.033	0.028	−0.099	0.012
	(0.028)	(0.046)	(0.062)	(0.046)
Length of Service	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes
Mean of dep. var.	0.19	0.26	2.82	3.17
Observations	561	557	574	571
R^2	0.16	0.18	0.15	0.25

Note: *Friends* equals one if partner reports that veteran seeks to control her relationships to friends; *Family* equals one if partner reports that veteran seeks to control her relationships with family members; *Support* measures how partner rates the degree of support of their children by the veteran (0–4); *Relationships* measures how partner rates the veteran's relationship with their children (0–4). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.7: Type and intensity of domestic violence

	Type of violence		Intensity of violence	
	(1) Physical	(2) Sexual	(3) Injury	(4) Fear
Wartime sexual violence	0.111**	0.004	0.058*	0.083***
	(0.055)	(0.003)	(0.034)	(0.026)
Length of Service	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes
Mean of dep. var.	0.45	0.15	0.27	0.57
Observations	586	586	586	575
R^2	0.15	0.13	0.21	0.13

Note: *Physical* equals one if partner reports physical domestic violence; *Sexual* equals one if partner reports sexual domestic violence; *Injury* equals one if partner reports at least one serious injury resulting from domestic violence; *Fear* equals one if partner reports that she is "often" or "very often" afraid of the veteran. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.8: Non-gender based violence

	Political violence			Jobs
	(1) Violence protesters	(2) Violence police	(3) Violence stay	(4) Violence job
Wartime sexual violence	−0.012	0.068**	−0.072**	0.004
	(0.061)	(0.028)	(0.031)	(0.016)
Length of Service	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes
Mean of dep. var.	0.23	0.13	0.16	0.06
Observations	555	554	545	578
R^2	0.12	0.12	0.17	0.16

Note: *Violence protesters* equals one if veteran thinks that protesters are justified to use violence in political demonstrations; *Violence police* equals one if veteran thinks that the police are justified to use violence against protesters; *Violence stay* equals one if veteran would stay at a protest that turns violent; *Violence job* equals one if respondent was engaged in either an illicit activity or work as a security guard in the last four weeks. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.9: Learning and unlearning

	Domestic violence							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wartime sexual violence								
... x Length of service	−0.000							
	(0.032)							
... x MPLA		0.005						
		(0.039)						
... x Time since left military			0.000					
			(0.064)					
... x Demobilized				−0.008				
				(0.036)				
... x DDR					0.056			
					(0.051)			
... x Factional ties						−0.054		
						(0.063)		
... x Village share of SV							−0.063**	
							(0.025)	
... x Village share of DV								0.002
								(0.024)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578	578
R^2	0.15	0.15	0.15	0.15	0.15	0.15	0.12	0.15

Note: All regressions include the main terms of both exposure to sexual violence and the interacted variable. The interacted variables are all standardized to zero mean and unit standard deviation. *Length of service* denotes length of wartime military service [in years]; *MPLA* equals one if main army was MPLA (zero means UNITA); *Time since left military* denotes time since respondent left the armed group [in years]; *Demobilization* equals one if respondent was demobilized in a formal process; *DDR* equals one if respondent was part of a formal DDR program; *Factional ties* equals one if respondent still has ties to former members of his units; *Village share of SV* denotes the share of veterans exposed to sexual violence (of sampled veterans in village); *Village share of DV* denotes the share of veterans reported to commit domestic violence (of sampled veterans in village). Regressions with village-level variables include comuna- instead of village-fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.10: Disputes between partners

	Deliberation			Disputes			Domestic violence	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Partner's income	Repro-duction	Extra-marital conflicts	Disputes	Starts disputes	More aggressive	Sample: disputes	Sample: no disputes
Wartime SV	−0.015	0.025	0.065***	0.264**	−0.020	−0.048	0.144***	−0.004
	(0.026)	(0.019)	(0.018)	(0.118)	(0.098)	(0.082)	(0.040)	(0.064)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.82	0.91	0.85	0.00	0.67	0.59	0.77	0.22
Observations	559	533	436	574	252	252	252	327
R^2	0.17	0.18	0.27	0.20	0.29	0.28	0.36	0.21

Note: *Partner's income*, *Reproduction*, and *Extra-marital conflicts* (i.e. conflicts with other people, such as over land) equals one if the couple reports that both partners are usually involved in making decisions in the respective domain (possibly together with other persons); *Disputes* measures the frequency of intra-partner disputes in the last 12 months reported by the partner (0–4), standardized to zero mean and unit standard deviation; *Starts disputes* equals one if partner reports that the veteran is more likely to start disputes than herself (if any disputes in the last 12 months); *Aggressive in disputes* equals one if partner reports that the veteran is usually more aggressive than herself in disputes (if any disputes in the last 12 months); *Sub-sample: disputes* means that regression was run for sub-sample of couples, where partner reports disputes; *Sub-sample: no disputes* means that regression was run for sub-sample of couples, where partner reports no disputes. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.11: Distress

	(1) Psychological distress	(2) Physical symptoms of distress	(3) Drinks alcohol	(4) Smokes cigarettes
Wartime sexual violence	−0.138	−0.172**	0.054*	−0.131***
	(0.093)	(0.077)	(0.029)	(0.040)
Length of Service	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes
Mean of dep. var.	−0.06	−0.03	0.49	0.31
Observations	578	578	578	578
R^2	0.29	0.27	0.20	0.18

Note: *Distress* is an index of psychological distress, evaluated via the Rotterdam Symptom Checklist (standardized). *Symptoms of distress* is an index of psyical symptoms of distress, evaluated via the Rotterdam Symptom Checklist (standardized). *Drinks alcohol* equals one if respondents reports thats he drinks alcohol, zero otherwise (never drinks). *Smoke cigarettes* equals one if respondent reports that he smokes cigarettes (or cigars), zero otherwise (never smokes). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4.12: Intensive margin

	Domestic violence				Psychological distress		Physical symptoms of distress	
	(1)	(2)	(3) Drop outliers	(4) Drop outliers	(5)	(6)	(7)	(8)
War sexual violence								
Moderate exposure	0.139**		0.139**		−0.246***		−0.256***	
	(0.062)		(0.062)		(0.091)		(0.088)	
High exposure	−0.000		−0.000		0.923***		0.676**	
	(0.143)		(0.143)		(0.312)		(0.294)	
Moderate exposure		0.146**		0.146**		−0.200**		−0.217***
		(0.061)		(0.061)		(0.086)		(0.075)
Extreme exposure		−0.235		−0.235		1.055**		0.746
		(0.167)		(0.167)		(0.533)		(0.496)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576	576	574	574	576	576	576	576
R^2	0.15	0.16	0.15	0.16	0.30	0.30	0.28	0.28

Note: *Situations of wartime sexual violence* denotes the absolute number of (diffferent) situations where a civilian woman was sexually abused, during wartime. *Distress* is an index of psychological distress, evaluated via the Rotterdam Symptom Checklist (standardized). *Symptoms of distress* is an index of physical symptoms of distress, evaluated via the Rotterdam Symptom Checklist (standardized). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Appendix A

Appendix Chapter 3

A.1 Data and variables

A.1.1 Data collection (POEMA)

The data and analysis are part of the Study of Angolan Ex-Combatants (POEMA), which integrates qualitative and quantitative components. Ethnographic fieldwork of 12 months started in January 2012, and was carried out by a social anthropologist from the University of Sussex (Spall, 2015). Quantitative fieldwork started in March 2013 and ended in February 2014, focused on survey data collection in partnership with the local Angolan NGO Development Workshop (DW). While NGO activity is generally very low in Angola, DW has operated in Angola – and especially Huambo Province – for more than 25 years, and was instrumental in making this survey possible.[1] With support from DW, the authors recruited, screened, trained, and managed a team of enumerators to conduct personal interviews based on electronic closed-ended questionnaires.

Sampled veterans had to complete two interviews. First, a private household-level interview together with their (main) cohabitant partner, if they had one at the time of the survey. Second, a private individual-level interview. The resulting data captures information from three different stages of a soldier's life: just before wartime military service, during wartime military service, and today, i.e. twelve years after the end of the war. The pre-service and service information is based on recall, and we worked with psychologists to reduce recall biases and optimize the reliability of the data. In Section A.1.5 we explain in more detail why we trust that our results are not driven by recall bias.

To ensure as representative a sample as possible, the survey employed three

[1]While well-known, DW have not implemented aid assistance or any development programs related to civic participation in Huambo province.

levels of randomization, where the first two involved the primary sampling unit (PSU) and enumeration area (EA) levels. A PSU (*bairro*) is in many rural areas the *comuna* (the lowest tier formal administrative unit), and in urban areas often equivalent to a borough. An EA is in rural areas either one settlement or multiple very small settlements, and in urban areas equivalent to a neighborhood. In the absence of systematic and reliable veteran population data, we used up-to date data of the total population from the ongoing Angolan census to randomly draw PSUs and EAs, with weights proportional to the population. At each survey site, we engaged with local chiefs, coordinators and administrations to produce listings of all former soldiers residing in the EA. Results were cross- and double-checked to develop credibly complete listings of the local veteran population. Conditional on the reliability of the general population as a proxy for the ex-combatant population, as used in the first stages, the sampling strategy is self-weighting and ensures that the geographic spread across the province is representative. Assuming that we did obtain complete lists of ex-soldiers, the EA-level sample is representative of the EA-level veteran population.

A.1.2 Data

Measures of planning and implementing the delivery of public goods (outcome). The outcome variables of interest are individual engagement in planning and implementing the delivery of local public goods. We follow the literature and use binary measures of participation (e.g. Bellows and Miguel, 2009), which are least prone to measurement error. We measure involvement in the planning stage via individual participation in the deliberative institutions where planning takes place: community meetings. Community meeting attendance is captured through a binary variable based on the question: "Did you attend any community meetings in the past year?" (1 if "Yes"; 0 otherwise). Participation in the delivery of public goods is captured through a binary variable based on the question: "Are you a member of a local self-security group or initiative?" (1 if "Yes"; 0 otherwise).

Measures of experience with wartime governance (treatment). The key explanatory variable is a soldier's experience with the wartime governance of civilians by their armed group. The index of wartime governance exposure is built from seven different items. The design of the survey instrument was guided by leading research on the effectiveness of governance by armed groups, which emphasizes the role of group capacity to deliver public strategic goods and services, such as security and dispute resolution; public technical goods and services, such as health and education; and mechanisms to foster civilian feedback in governmental issues (Mampilly, 2011; Arjona, Kasfir and Mampilly, 2015). The seven specific items were identified by ethnographic research that preceded the survey, informal interviews, and extensive pre-testing. During the interview, survey respondents were

asked about the overall frequency with which they experienced certain aspects of local governance during their time with the armed group. We provide detailed information on the seven items below.

Service provision. This variable indicates individual answers to the question: "Overall, how frequently did you/your unit provide or help with access to schooling, water, electricity, communication or deliver related services?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Building infrastructure. This variable indicates individual answers to the question: "Overall, how frequently did you/your unit build local facilities, such as schools, health posts, community centers, or buildings for religious purposes?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Provision of arms. This variable indicates individual answers to the question: "Overall, how frequently did you/your unit provide arms to civilians to defend themselves?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Help with conflict resolution. This variable indicates individual answers to the question: "Overall, how frequently did you/your unit help with the resolution of disputes between civilians, including conflicts over land or water?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Protection and security. This variable indicates individual answers to the question: "Overall, how frequently did you/your unit protect civilians from war violence/attacks by armed groups?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Requests for help with conflict resolution. This variable indicates individual answers to the question: "Overall, how frequently did villagers approach/turn to you/your unit for help with the resolution of disputes between civilians, including conflicts over land or water?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Requests for protection and security. This variable indicates individual answers to the question: "Overall, how frequently did villagers approach/turn to you/your unit for protection of civilians from war violence/attacks by armed groups?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

A.1.3 Index of experience with wartime governance

We use a summary index built from the seven wartime governance indicators for two reasons. First, an aggregate measure is potentially more powerful statistically than individual-level tests as it may emphasize common patterns in the mass distributions of the raw measures. Individual effects that are marginally significant may aggregate to statistical significance of the aggregate measure. Second, an index may improve

statistical performance by mitigating concerns of and smoothing over measurement error in the items.

While we expect that some of the dimensions of wartime governance above will be substantially correlated, we hypothesize that each item captures a *different* aspect of governance, irrespective of its correlation with other items. We have therefore constructed an index that assigns equal weights to all items, which is technically similar to previously used indices of victimization (Voors et al., 2012; Bellows and Miguel, 2009). Alternative approaches calculate weights based on the variance-covariance matrix of the domains, including principal components (Filmer and Pritchett, 2001) and the approach suggested by Anderson (2008). In contrast to assigning equal weights, these approaches are implicitly based on the assumption that highly correlated items measure 'the same aspect' and 'penalize' a given item based on intercorrelations with other item. As our index is based on frequency measures of exposure (and we thus include length of service as a control variable in standard specifications), an additional option is to directly adjust the standard index for length of service. As robustness checks, we therefore discuss results from the alternative weighting schemes and the tenure-adjusted index, and also investigate the effects and influence of each item separately in Section A.2.1.

A.1.4 Summary statistics

Table A.3 reports summary statistics for our main variables of interest and principal control variables. The statistics show that there is considerable variability in experience with wartime governance. The mean respondent has a wartime governance experience score of 1.16 (SD=.77). The mean score of a respondent above the 75th percentile of the score distribution is 2.35 (SD=.37). Similarly, we observe substantial variation in engagement in public good production today. About 22% of all respondents attend community meetings, while 6% contribute to local security. The low rate of participation in local security provision can be interpreted as support for our assumption that participation is voluntary and unpaid. Summary statistics for the individual components of the wartime governance index and selected related non-violent wartime interactions are reported in the bottom panel of Table A.3.

A.1.5 Recall bias

Recall bias poses a threat to any study using retrospective data, and one key source of error is misreporting due to incorrect memory and lack of knowledge. A main concern is that respondents may misreport because they need to recall information and situations from a long time ago, or because they did not perceive these as important. Psychologists have emphasized and developed data collection techniques based on the premise that individuals may recall information better when they can relate them temporally to incisive events (e.g Freedman et al., 1988). Being enlisted

into the military is a defining life event that should hence increase a respondent's ability to recall outcomes just before this happened reasonably accurately. Similarly, situations and events during military service are plausibly incisive experiences themselves and therefore less prone to memory failure. Two observations from fieldwork strengthen the assumption that the recalled service and pre-service information is reliable. First, "I don't know" or "I can't remember" responses were very rare. Second, the complementary ethnographic research by Spall (2015) gathered vast and detailed information on 'life before the military' and 'life in the military', which all interview partners were able and willing to provide.

A second important concern is that respondents may misreport their year of birth, which is a key variable in the study. To address this issue, we asked for information on date of birth and current age separately, in separate interviews. In the initial household interview with the veteran and his partner (if in a partnership) information on the ages of all household members was collected, including the veteran's. In the individual interview with the veteran, we asked for his date of birth. If necessary, enumerators provided help to determine the year of birth relying on an extensive list of important events in Angolan history. This entry was then immediately compared to the age entry (from the first interview) by the enumerator. For inconsistent answers, enumerators revisited the questions and provided assistance in determining as correct answers as possible. As an additional quality test, we check for 'age heaping', in which case answers tend to be rounded to multiples of 5 years, and find no evidence for such patterns (Figure A.1).

A final concern is that individuals may misreport for personal reasons, which is especially relevant for sensitive and traumatic experiences. A well-documented behavior is the "embroidery" of personal experiences in such cases (e.g Mausner and Kramer, 1985). Yet, veterans exhibited an overwhelming openness toward discussing their service and pre-service lives in both qualitative and quantitative interviews. In the quantitative component, no respondent opted to not answer questions regarding pre-military life, skip questions or quit the interview. As expected, a few respondents did opt to skip specific questions on military life, but the number of such cases was low and not systematic, i.e. not concentrated in specific questions or respondent characteristics. For these reasons, we trust that the vast majority of the questions were not particularly sensitive (in this context) or difficult to answer, and that our data are not affected by systematic misreporting. Even if misreporting were systematic, it seems unlikely that this would occur in a way that would affect our main statistics and estimates.

A.2 Additional results

A.2.1 OLS

To account for potential intra-cluster correlation in the error term, we report for our main specifications Cameron, Gelbach and Miller (2011)-type standard errors, two-way clustered at the PSU (N=22) and municipality of recruitment (N=38) levels. Table A.4 presents p-values of our coefficient of interest based on alternative standard errors. In column 1, standard errors are one-way clustered by the location of current residence. Because the numbers of clusters may be 'small' (e.g. Cameron, Gelbach and Miller, 2011), a potential concern is that we incorrectly inflate (or deflate) standard errors as a consequence of clustering. Column 2 presents p-values based on one-way clustered standard errors parametrically corrected by the Moulton-method (e.g. Angrist and Pischke, 2008), column 3 reports standard errors based on wild cluster bootstrapping.[2] In Columns 4 and 5 we report Huber-White and classical standard errors. The estimated standard errors do not vary noticeably across these methods, and all result in comparable confidence intervals with p-values (well) below .01.

As the outcome variables are dichotomous, we test whether the main result is robust to non-linear model specifications. Table A.5 reports average marginal effects estimated for a logit model. Across specifications, these are very similar – in significance and magnitude – to the coefficients in the linear probability model.

Next, we investigate results for the continuous index, alternative weighting schemes for its construction, and the effects of each individual index item. We find that the positive association with post-war participation holds for the continuous wartime governance indices, and demonstrate its robustness to three alternatively constructed indices, including principal component analysis, the method suggested by Anderson (2008), and a tenure-adjusted standard index (Table A.6). The standardized coefficients and standard errors suggest that our results are not sensitive to how the index measure is built.

While an index is advantageous for reasons presented earlier, the aggregate measure may mask interesting differences in impact across items. Table A.7 reveals that the intercorrelations between the items are markedly moderate in magnitude, ranging from .17 to .50. This corroborates our assumption that each experience covers a different aspect of governance and may not necessarily occur at the same time as others. Table A.8 displays the results from regressions of the outcomes on the disaggregated (standardized) items separately, using standard specifications. We observe that each single component is a positive predictor of both steps of public

[2]We use Rademacher weights (+1 with probability 0.5 and 1 with probability 0.5) for re-sampling residuals and impose that the null hypothesis of zero treatment effect as recommended by Cameron, Gelbach and Miller (2008). While frequently used, it should also be noted that this method is only reliable for large sample sizes.

good production and statistically significant, with the exception of service provision on community meeting attendance. This result also shows that our main result is not driven primarily by responses to a single question. As all items are standardized, we also observe that the magnitude of the disaggregated effects is relatively similar across items.

A.2.2 IV

In our main IV-specification we rely on a single instrument, which is favorable in terms of approximate bias (Hahn and Hausman, 2003; Angrist and Pischke, 2008). Yet, if our IV-strategy is valid, alternative instrument specifications based on the interaction of the relevant UNITA and bin dummy variables should give similar results. Table A.14 shows that the coefficients are stable across two alternative instrument specifications for both outcomes. Using the interaction with bin II and IV additively (columns 2 and 5), as well as adding the interaction with bin III (columns 3 and 6), produces very similar results.

In our main IV-specification we also left out the 'violence received' variable, shifting it to the error term. As a last robustness check for the validity of the IV results, we turn to models that include violence received as an endogenous regressor. Based on the history of the war and the logic that underlies our IV strategy, it is plausible that the interaction of UNITA with bin III may be positively correlated with individual violence received. Table A.15 confirms this intuition, and shows that the strongly significant correlation is robust to the inclusion of the Bin24-factorial (column 3) and post-war outcomes (column 4). The F-statistic of the instrument is moderate and ranges from 5.8 to 7.5. Yet, as our effect of interest is that of individual exposure to wartime public good provision, finite-sample bias of the coefficient on violence received is not a first-order concern.

Table A.16 displays IV- and corresponding OLS-estimates for community meeting attendance and security provision based on models that include the violence received variable. Columns 2, 4, 6 and 8 include both endogenous regressors and we report standard, Sanderson-Windmeijer and Angrist and Pischke F-statistics for the IV specifications (Sanderson and Windmeijer, 2016; Angrist and Pischke, 2008).[3] For our purposes, the key observation is that these models confirm the robust and positive effect of wartime governance on post-war participation we found in the main analysis. Specifically, the inclusion of violence received does not markedly change the magnitude and statistical significance of the estimate from the main analysis without the violence received variable.

The impact of violence received on participation is not the focus of this study, but we observe a positive correlation with post-war participation, which, however,

[3]These should be interpreted with caution as we estimate models with clustered standard errors while the basis for these test statistics are models with i.i.d. errors.

disappears when we include the wartime governance measure, both for OLS and IV estimation. While only suggestive, these results contrast with positive effects on social participation and behavior found in previous studies (e.g. Bellows and Miguel, 2009; Bauer et al., 2016), where endogeneity of this variable is usually not directly accounted for. At the same time, it is important to note that our results for violence received need to be interpreted with caution. Most importantly, the instrument for violence is not 'strong' in the sense of Staiger and Stock (1997), and may not be valid. Yet, overall these results add to some recent evidence that casts some doubt on a strongly positive, robust, and dominant impact of the exposure to violence on social participation and behavior (Adhvaryu and Fenske, 2014; Gáfaro, Ibáñez and Justino, 2014).

A.2.3 Village level

An interesting and important question from a development point of view is whether the behavioral relationships we isolated at the individual level scale up to more aggregate levels. We thus explore whether the average contribution to public good production is higher in a village with a higher share of 'high-exposure' types in the village-sample. If high-exposure types strongly cluster in certain villages, the sample may include 'extreme' villages with shares of zero or one. Figure A.4 reveals that we do not observe such villages, and the village-level shares of high-exposure types are relatively narrowly distributed around the overall individual mean of being a high-exposure type of about one quarter.

Simple linear regression suggests that the village-level share of high types is positively associated with the village-level mean of contributing to public good production. Figure A.5 shows scatter plots of the residuals and linear projections. The correlations are estimated at .3 for community-meeting attendance and .2 for local security provision. The effects are not statistically significant, which is not surprising given a sample size of 34 and the inclusion of fixed effects and robust standard errors clustered at the *comuna*-level. The positive relationship, however, strengthens confidence that the effect of interest is present and relevant.

A.3 Additional figures and tables

Figure A.1: Distribution of year of birth

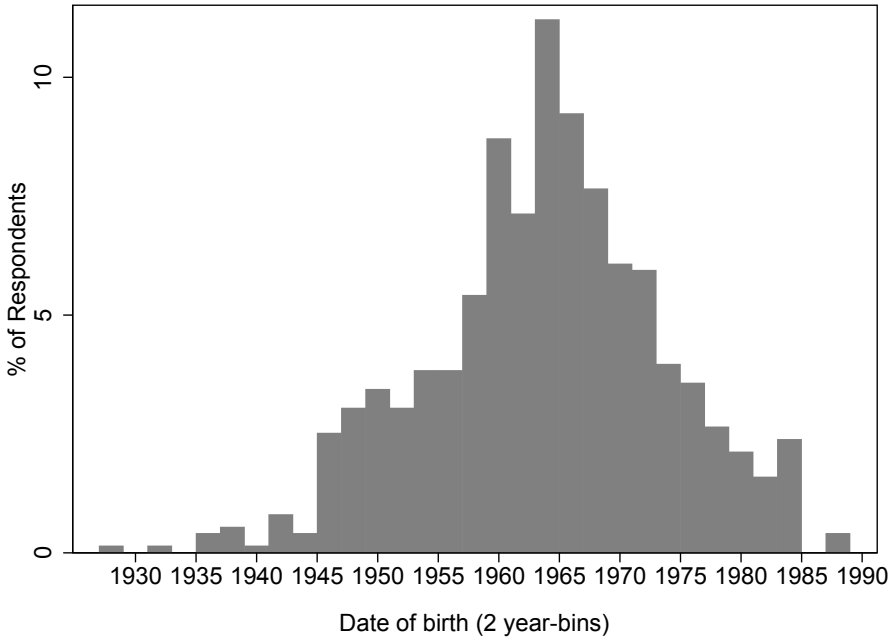


Figure A.2: Exposure to wartime governance as a categorical variable

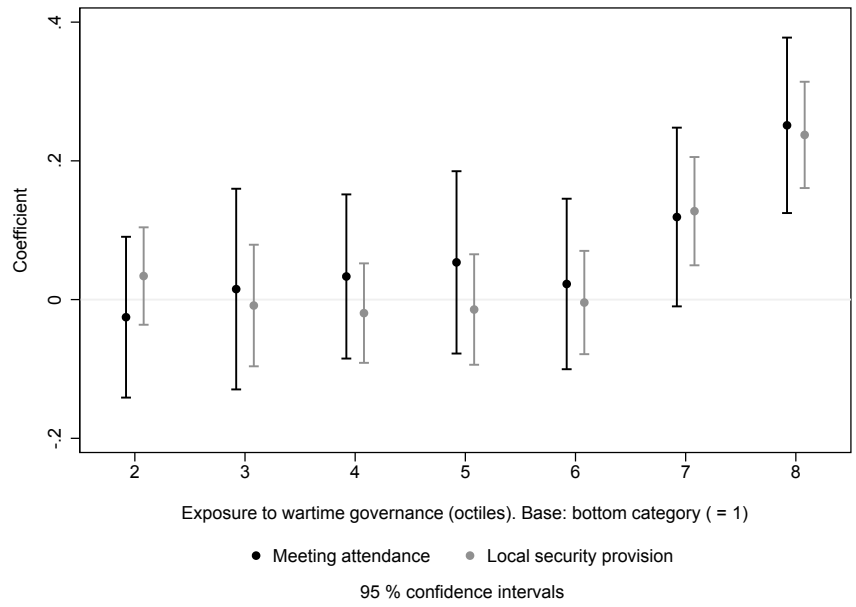
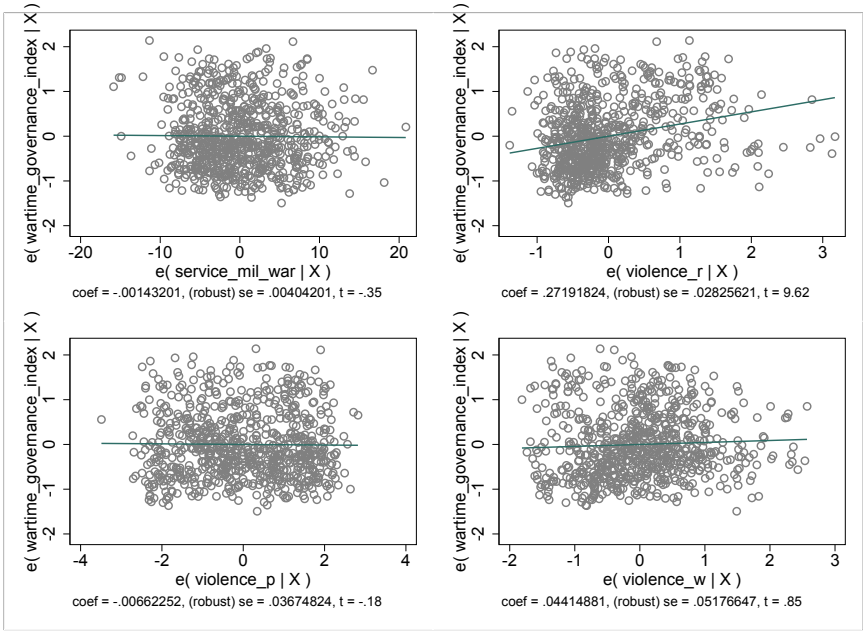


Figure A.3: Residual-residual plots (other war experiences)



Note: In each plot all standard regressors were partialled out. Top left: wartime military service in years. Top right: Violence received, measured by an average index based on 9 violent acts. Bottom left: Violence perpetrated, measured by an average index based on 3 violent acts. Bottom right: Violence witnessed, measured by an average index based on 5 violent acts. All 17 violent acts measured via a five-point frequency scale, ranging from never = 0 to extremely often = 4, based on survey instruments calibrated by a team of psychologists.

Figure A.4: Histogram of share of high exposure types in village

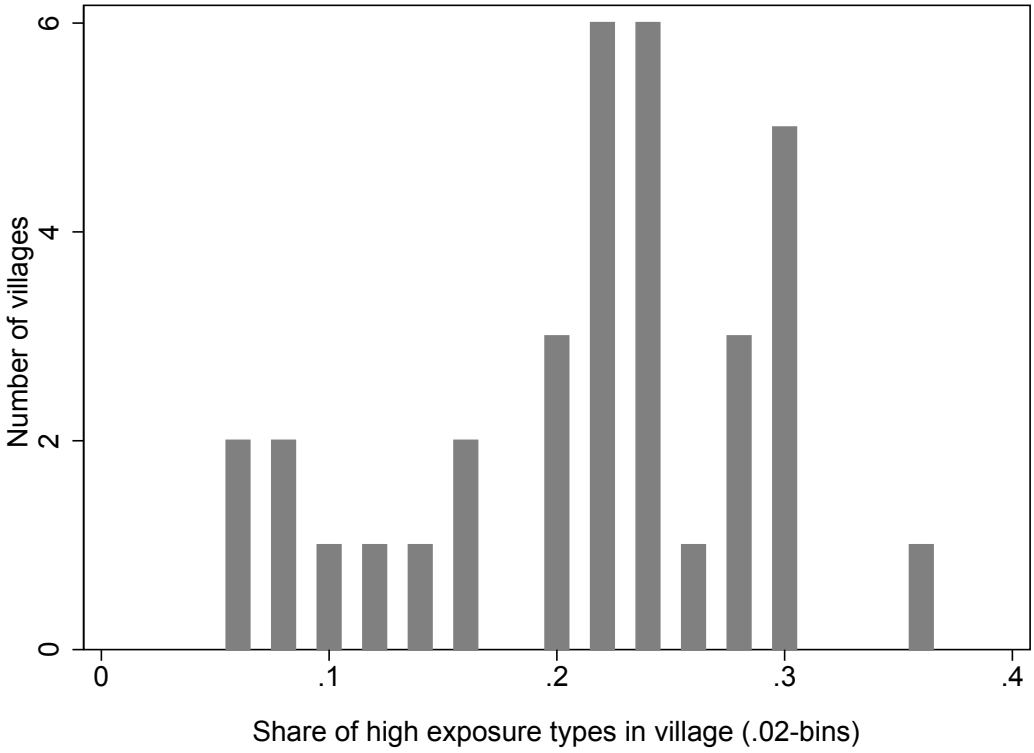
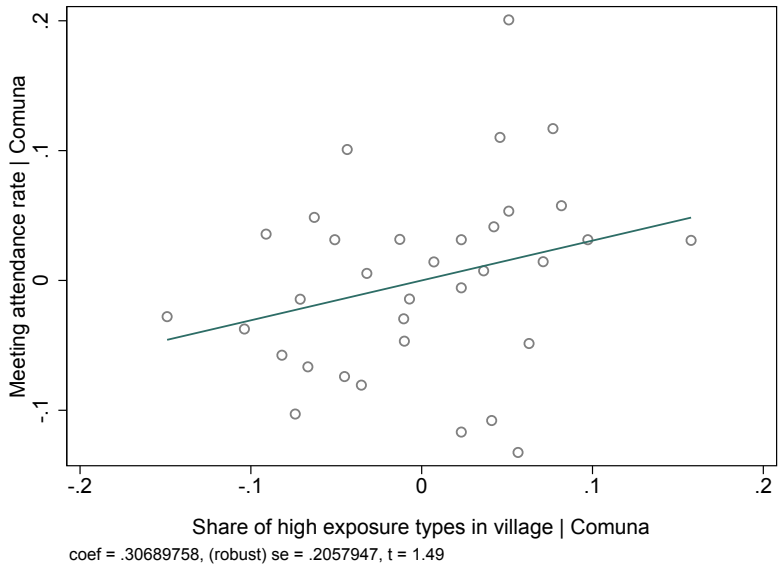


Figure A.5: Village level

(a) Village level: participation in community meetings.



(b) Village level: participation in local security groups.

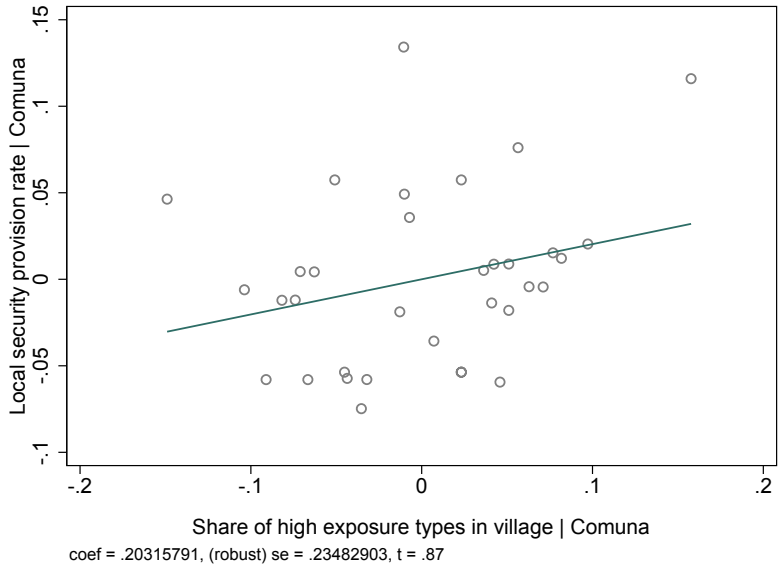


Table A.1: Wartime interactions with civilians (ever)

	UNITA	MPLA
Exposure to wartime governance (ever)		
Services	0.52	0.60
Infrastructure	0.61	0.61
Arms	0.23	0.16
Conflict resolution	0.49	0.52
Protection	0.81	0.90
Req: conflict resolution	0.36	0.34
Req: protection	0.39	0.31
Other interactions with civilians (ever)		
Collected taxes	0.36	0.31
Attended social events together	0.50	0.51
Taught political ideas	0.70	0.63
Observations	226	534

Note: Entries indicate the share of respondents who ever experienced the specific interaction when they were in the military.

Table A.2: Selection into armed group

	UNITA first army	
	(1)	(2)
Household size	−0.001	−0.001
	(0.842)	(0.800)
Church = Catholic (d)	−0.021	−0.068
	(0.726)	(0.252)
Church = IECA (d)	−0.031	−0.021
	(0.635)	(0.746)
Number of rooms	−0.001	−0.013
	(0.966)	(0.345)
Land size	−0.001	0.000
	(0.886)	(0.962)
Livestock	−0.002	−0.003
	(0.347)	(0.216)
Farmer(d)	0.061	−0.001
	(0.381)	(0.987)
Radio (d)	−0.030	−0.015
	(0.380)	(0.644)
HH members had joined (#)	−0.013	−0.018
	(0.362)	(0.169)
Ever displaced (d)	−0.042	−0.038
	(0.238)	(0.265)
Father's schooling (d)	−0.025	−0.047
	(0.532)	(0.201)
Mother's schooling (d)	−0.004	−0.007
	(0.914)	(0.853)
Pre-Location FE	No	Yes
Observations	760	760

Note: Pre-Location FE: full set of recruitment region fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Classical standard errors.

Table A.3: Key summary statistics

	Mean	SD	MIN	MAX
Main outcome: Participation in public good production				
Community meetings	0.22	0.417	0	1
Local security provision	0.06	0.246	0	1
Main explanatory variable: Exposure to wartime governance				
Exposure to wartime governance	1.16	0.772	0	3
... (in top quartile)	2.35	0.366	2	3
Armed group and pooled birth cohorts				
UNITA (main)	0.30	0.457	0	1
YOB $\leq$ 1962	0.43	0.496	0	1
1963 $<$ YOB $\leq$1971	0.38	0.485	0	1
1972 $<$ YOB $\leq$1975	0.09	0.282	0	1
YOB $>$1975	0.10	0.304	0	1
War control variables				
Length of service	11.15	6.806	1	40
Violence witnessed	1.49	0.853	0	4
Violence received	0.84	0.746	0	4
Violence perpetrated	2.13	1.415	0	4
Socio-economic control variables				
Radio	0.74	0.440	0	1
Wealth	0.00	2.501	-2	26
Education	0.85	0.361	0	1
Born in this comuna	0.74	0.439	0	1
Age	49.73	9.847	25	86
Wartime governance components				
Services	1.29	1.349	0	4
Infrastructure	1.52	1.526	0	4
Arms	0.40	0.949	0	4
Conflict resolution	1.07	1.274	0	4
Protection	2.46	1.329	0	4
Civilian requests for conflict resolution	0.70	1.137	0	4
Civilian requests for protection	0.67	1.092	0	4
Other non-violent interactions with civilians				
Taxes on economic activity	0.73	1.231	0	4
Attended social events together	0.92	1.128	0	4
Taught political ideas	1.54	1.372	0	4
Observations	760			

Note: Binary measures: Community meetings, Local security, UNITA (main), Radio, Education, and Born here (1 = Yes, 0 = No). *Index measures*: Wartime governance (7 items), Violence witnessed (5), Violence received (9), Violence perpetrated (3), Wealth (20). *In years*: Length of service, Age. *Frequency measures*: Services, Infrastructure, Arms, Conflict, Resolution, Protection, Civilian requests for conflict resolution, Civilian requests for protection, Collected taxes, Attended social events together, Taught political ideas (4 = Extremely often,..., 0 = Never.)

Table A.4: Robustness: Alternative standard errors

	(1) CGM 1-Way	(2) Moulton	(3) WCB	(4) Huber-White	(5) Classical
Panel A: Community meeting attendance					
WG high coefficient	0.173				
p-val	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel B: Local security provision					
WG high coefficient	0.153				
p-val	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760

Note: Estimation of standard errors: one-way clustered by *bairro* (col. 1), Moulton-corrected (col. 2), wild-cluster bootstrapped (col. 3), Huber-White-corrected (col. 4), classical (col. 5). *WG high*: binary indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: binary indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of binary indicators of year of birth bin (1 = Yes, 0 = No). *Post-Location FE*: full set of *comuna* fixed effects.

Table A.5: Non-linear model specifications

	Community Meetings				Local Security			
	(1) OLS	(2) OLS	(3) AME	(4) AME	(5) OLS	(6) OLS	(7) AME	(8) AME
WG high	0.161***	0.159***	0.165***	0.161***	0.152***	0.151***	0.199***	0.196***
	(0.039)	(0.039)	(0.039)	(0.039)	(0.030)	(0.030)	(0.037)	(0.036)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	No	Yes	No	Yes	No	Yes	No
Post-Location FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	760	760	760	760	760	760	760	760

Note: Average marginal effects are reported. *Estimation*: OLS (cols. 1, 2, 5, 6), logit (cols. 3, 4, 7, 8). *UNITA (main)*: binary indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of binary indicators of year of birth bin (1 = Yes, 0 = No). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location*: full set of recruitment region fixed effects. *Post-Location*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Huber-White standard errors in parentheses, fixed effects as in standard specifications (logit results to be analyzed with caution).

Table A.6: Alternatively constructed indices

	Community Meetings				Local Security			
	(1) Ind1	(2) Ind2	(3) Ind3	(4) Ind4	(5) Ind1	(6) Ind2	(7) Ind3	(8) Ind4
WG index	0.063***	0.068***	0.067***	0.061***	0.054***	0.059***	0.058***	0.044***
	(0.015)	(0.019)	(0.017)	(0.013)	(0.010)	(0.009)	(0.011)	(0.012)
UNITA+Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760

Note: Standardized coefficients; all indices standardized to mean zero and unit standard deviation. Ind1: standard index (cols. 1, 5). Ind2: index based on principal component analysis (cols. 2, 6). Ind3: index based on the method described in Anderson (2008) (cols. 3, 7). Ind4: standard index adjusted for length of service (cols. 4, 8). *UNITA (main)*: dummy indicating if main armed group was UNITA (zero means MPLA). *YOB-Bin*: dummies indicating pooled year of birth cohort. *Post-Location*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A.7: Wartime governance index components: correlation

	Services	Infra-structure	Arms	Conflict resolution	Pro-tection	Req: conflict resolution	Req: protection
Services	1.000						
Infrastructure	0.503	1.000					
Arms	0.171	0.316	1.000				
Conflict resolution	0.180	0.278	0.383	1.000			
Protection	0.354	0.403	0.201	0.247	1.000		
Req: conflict resolution	0.118	0.164	0.302	0.374	0.183	1.000	
Req: protection	0.102	0.141	0.410	0.414	0.195	0.591	1.000

Note: Req: conflict resolution: civilians requested help with conflict resolution. *Req: protection*: civilians requested protection.

Table A.8: Wartime governance index components: impact

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Community meetings attendance							
Services	0.009						
	(0.009)						
Infrastructure		0.026*					
		(0.014)					
Arms			0.047**				
			(0.021)				
Conflict resolution				0.050***			
				(0.013)			
Protection					0.043***		
					(0.008)		
Req: conflict resolution						0.042**	
						(0.019)	
Req: protection							0.041**
							(0.016)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760
R^2	0.10	0.10	0.11	0.11	0.11	0.10	0.10
Panel B: Participation in local security groups							
Services	0.022***						
	(0.008)						
Infrastructure		0.022**					
		(0.010)					
Arms			0.051***				
			(0.015)				
Conflict resolution				0.036***			
				(0.011)			
Protection					0.023***		
					(0.004)		
Req: conflict resolution						0.055***	
						(0.009)	
Req: protection							0.037***
							(0.013)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760
R^2	0.07	0.08	0.11	0.09	0.08	0.11	0.09

Note: Standardized measures, with mean zero and unit standard deviation. *Std. controls*: Same specification as in column 1 of Table 5. *Req: conflict resolution*: civilians requested help with conflict resolution. *Req: protection*: civilians requested protection. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A.9: Are military or background traits correlated with treatment?

	Sample			Difference	
		High exposure			
	Full	Yes	No	Diff	*p*
Background					
Farming household (d)	0.94	0.94	0.93	0.00	0.87
Catholic household (d)	0.65	0.64	0.65	−0.01	0.90
Father any schooling (d)	0.57	0.54	0.57	−0.03	0.49
Mother any schooling (d)	0.39	0.40	0.38	0.01	0.81
Household size (#)	7.48	7.73	7.41	0.33	0.22
HH members recruited (#)	0.90	0.94	0.88	0.05	0.62
HH members killed (#)	0.15	0.17	0.14	0.03	0.30
Subjective health (1-10)	6.15	6.43	6.08	0.35	0.14
Any training (d)	0.06	0.04	0.07	−0.03	0.18
Any schooling (d)	0.64	0.73	0.61	0.12***	0.00
Schooling (yrs)	2.43	2.82	2.32	0.50**	0.02
Armed group and pooled birth cohorts					
UNITA (main)	0.30	0.30	0.30	0.01	0.87
YOB $\leq$ 1962	0.43	0.46	0.43	0.03	0.51
1963 $<$ YOB $\leq$1971	0.38	0.40	0.37	0.02	0.58
1972 $<$ YOB $\leq$1975	0.09	0.07	0.09	−0.02	0.34
YOB $>$1975	0.10	0.08	0.11	−0.03	0.29
Military					
Any training (d)	0.97	0.96	0.97	−0.00	0.82
Training (weeks)	15.38	19.35	14.31	5.04***	0.00
Role: infant (d)	0.43	0.36	0.44	−0.08*	0.07
Rank: private (d)	0.47	0.44	0.48	−0.04	0.40
Combat zone (d)	0.77	0.86	0.75	0.11***	0.00
Battallions (#)	2.14	2.40	2.07	0.33*	0.05

Note: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Are military or background traits correlated with binary instrument?

	Sample			Difference	
		High exposure			
	Full	Yes	No	Diff	p
Background					
Farming household (d)	0.94	0.95	0.93	0.02	0.54
Catholic household (d)	0.65	0.68	0.64	0.04	0.39
Father any schooling (d)	0.57	0.52	0.58	−0.06	0.24
Mother any schooling (d)	0.39	0.38	0.39	−0.01	0.86
Household size (#)	7.48	7.28	7.51	−0.23	0.45
HH members recruited (#)	0.90	0.75	0.92	−0.17	0.16
HH members killed (#)	0.15	0.17	0.14	0.03	0.41
Subjective health (1-10)	6.15	5.98	6.18	−0.20	0.45
Any training (d)	0.06	0.09	0.05	0.04*	0.08
Any schooling (d)	0.64	0.59	0.65	−0.05	0.29
Schooling (yrs)	2.43	2.14	2.48	−0.34	0.15
Military					
Any training (d)	0.97	0.97	0.97	−0.00	0.99
Training (weeks)	15.38	14.19	15.60	−1.41	0.20
Role: infant (d)	0.43	0.47	0.42	0.05	0.35
Rank: private (d)	0.47	0.41	0.48	−0.07	0.16
Combat zone (d)	0.77	0.78	0.77	0.01	0.89
Battallions (#)	2.14	2.09	2.15	−0.06	0.76

Note: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Correlation of IV with pre-service variables

	UNITA x YOB-Bin2/4		UNITA first army			
	(1)	(2)	(3)	(4)	(5)	(6)
			Bin2/4	Bin2/4	Bin2/4	Bin2/4
Household size	0.000	0.000	−0.003	0.002	−0.005	0.003
	(0.959)	(0.996)	(0.746)	(0.780)	(0.457)	(0.560)
Church = Catholic (d)	−0.040	−0.052	−0.079	0.032	−0.106	0.066
	(0.407)	(0.353)	(0.363)	(0.707)	(0.381)	(0.250)
Church = IECA (d)	−0.074	−0.087	−0.093	0.031	−0.114	0.086
	(0.154)	(0.166)	(0.338)	(0.727)	(0.417)	(0.391)
Number of rooms	−0.020*	−0.021	−0.025	0.016	−0.023	0.017
	(0.082)	(0.218)	(0.251)	(0.411)	(0.376)	(0.233)
Land size	−0.003	−0.003	−0.014	0.019	−0.008	0.014
	(0.683)	(0.564)	(0.217)	(0.166)	(0.136)	(0.220)
Livestock	−0.001	−0.002	−0.003	−0.001	−0.003	−0.004
	(0.444)	(0.148)	(0.295)	(0.666)	(0.105)	(0.169)
Farmer (d)	0.009	−0.014	0.024	0.080	−0.057	0.118
	(0.869)	(0.850)	(0.818)	(0.404)	(0.692)	(0.144)
Radio (d)	0.019	0.030	−0.024	−0.055	−0.017	−0.065
	(0.482)	(0.322)	(0.649)	(0.246)	(0.589)	(0.259)
Members joined (#)	−0.013	−0.012	−0.030	0.009	−0.028	0.010
	(0.233)	(0.143)	(0.146)	(0.657)	(0.121)	(0.404)
Ever displaced (d)	−0.013	−0.004	−0.032	−0.057	−0.011	−0.046
	(0.634)	(0.919)	(0.550)	(0.251)	(0.872)	(0.227)
Father's schooling (d)	−0.030	−0.036	−0.071	0.022	−0.069	0.020
	(0.328)	(0.342)	(0.215)	(0.686)	(0.283)	(0.722)
Mother's schooling (d)	0.017	0.005	0.014	−0.018	−0.000	−0.047
	(0.574)	(0.868)	(0.809)	(0.738)	(1.000)	(0.296)
Pre-Location FE	No	Yes	No	No	Yes	Yes
Observations	760	760	364	396	364	396
R^2	0.02	0.06	0.04	0.02	0.14	0.09

Note: *Pre-Location FE*: full set of recruitment region fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses; in columns 1, 3 and 4 classical; in columns 2, 5 and 6 robust, clustered at the pre-service location level.

Table A.12: Correlation of IV with violence

	General indices			Received in battle		
	(1) Violence received	(2) Violence witnessed	(3) Violence perpetrated	(4) In line of fire	(5) Severely injured	(6) Many lose life
UNITA x YOB-Bin2/4	−0.118	0.041	−0.110	0.087	−0.117	−0.083
	[0.472]	[0.803]	[0.504]	[0.596]	[0.476]	[0.616]
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	756	757	748
R^2	0.01	0.01	0.01	0.00	0.01	0.01

Note: Dependent variables are standardized to zero mean and unit standard deviation, and based on survey instruments calibrated by a team of psychologists. General measures are indices based on a set of specific acts: *Violence received (9 acts)*, *Violence perpetrated (5 acts)*, *Violence witnessed (5 acts)*. All 17 violent acts measured via a five-point frequency scale, ranging from never = 0 to extremely often = 4. Specific measures of extreme violence received in the battlefield: *In the line of fire*, *Severely injured*, *Many (companions) lose life*. All three variables as measured via a scale of total incidence (number of situations). *UNITA (main)*: binary indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of binary indicators of year of birth bin (1 = Yes, 0 = No). *YOB-Bin 2/4* collapses *YOB-Bin 2* and *YOB-Bin 4* into a joint bin. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01; p-values in brackets. Classical standard errors.

Table A.13: Results for full index of exposure

	Community Meetings		Local Security	
	(1) OLS	(2) IV	(3) OLS	(4) IV
WG index	0.057***	0.152**	0.054***	0.113*
	(0.015)	(0.075)	(0.010)	(0.060)
Std. controls	Yes	Yes	Yes	Yes
Observations	760	760	760	760
F		12.96		12.96

Note: Standardized measure of exposure to wartime governance, with mean zero and unit standard deviation. *WG index*: full index measure of exposure to wartime governance. *Std. controls*: same specification as in column 1 of Table 5. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A.14: IV: alternative instruments (no pre-service controls)

	Community meetings			Local security		
	(1)	(2)	(3)	(4)	(5)	(6)
	24	2+4	2+3+4	24	2+4	2+3+4
WG high	0.398***	0.386***	0.383***	0.255**	0.251**	0.248**
	(0.140)	(0.140)	(0.138)	(0.119)	(0.119)	(0.116)
UNITA+Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes
Post-Controls	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760
R^2	0.04	0.05	0.05	0.06	0.06	0.06
F-stat(IV)	48.59	30.81	21.71	48.59	30.81	21.71

Note: *WG high*: binary indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: dummy indicating if main armed group was UNITA (zero means MPLA). *YOB-Bin*: dummies indicating pooled year of birth cohort. *Post-Controls*: radio ownership (dummy), assets (index), years of schooling, born in this comuna (dummy). *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A.15: Violence received: First-stage mechanics

	Violence received			
	(1)	(2)	(3)	(4)
UNITA x YOB-Bin 3	0.335***	0.383**	0.347**	0.388**
	(0.123)	(0.151)	(0.138)	(0.161)
UNITA x YOB-Bin2/4			0.022	0.008
			(0.081)	(0.100)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes
Post-Controls	No	Yes	No	Yes
Pre-Controls	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes
Post-Location FE	No	Yes	No	Yes
Observations	760	760	760	760
R^2	0.08	0.11	0.08	0.11
F(UNITA x YOB-Bin3 = 0)	7.46	6.41	6.29	5.79

Note: *UNITA (main)*: dummy indicating if main armed group was UNITA (zero means MPLA). *YOB-Bin x*: binary indicator of year of birth bin (1 = Yes, 0 = No). The sample is grouped into four bins (1 – 4), as explained in the text. *YOB-Bin 1* is the oldest group and the reference bin, YOB-Bin 4 is the youngest group. *YOB-Bin 2/4* collapses *YOB-Bin 2* and *YOB-Bin 4* into a joint bin. *YOB-Bin*: full set of binary indicators of year of birth bin (1 = Yes, 0 = No). *Post-Controls*: radio ownership (dummy), assets (index), years of schooling, born in this comuna (dummy). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, in columns 1 and 3 clustered by municipality of recruitment, in columns 2 and 4 two-way clustered by *bairro* of residence and municipality of recruitment.

Table A.16: Robustness check: Violence received (OLS and IV)

	Community Meetings				Local Security			
	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) OLS	(6) OLS	(7) IV	(8) IV
Violence received	0.052**	0.030	0.105	−0.014	0.046***	0.024	0.096	0.022
	(0.021)	(0.023)	(0.255)	(0.230)	(0.015)	(0.016)	(0.154)	(0.159)
WG high		0.139***		0.349**		0.140***		0.214*
		(0.043)		(0.169)		(0.037)		(0.122)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760
R^2	0.13	0.15	0.12	0.11	0.09	0.13	0.07	0.12
F- VR			5.85	2.92			5.85	2.92
F-SW VR			5.85	4.92			5.85	4.92
F-AP VR			5.85	5.31			5.85	5.31
F- WG				153.61				153.61
F-SW WG				35.62				35.62
F-AP WG				198.74				198.74

Note: F-VR: 1st stage F-statistic "Violence received", *F-SW VR*: Sanderson-Windmeijer F-statistic "Violence received", *F-AP VR*: Angrist-Pischke F-statistic "Violence received". *F-WG*: 1st stage F-statistic "WG high", *F-SW WG*: Sanderson-Windmeijer F-statistic "WG high", *F-AP WG*: Angrist-Pischke F-statistic "WG high". *WG high*: binary indicator of high exposure to wartime governance (1 = Yes, 0 = No). *WG high*: binary indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: binary indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of binary indicators of year of birth bin (1 = Yes, 0 = No). *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, in columns 1 and 3 clustered by municipality of recruitment, in columns 2 and 4 two-way clustered by *bairro* of residence and municipality of recruitment.

Table A.17: Mechanisms: heterogeneity in age at entry

	Community Meetings			Local Security		
	(1)	(2)	(3)	(4)	(5)	(6)
Age at military entry	<18	18-24	>24	<18	18-24	>24
WG high	0.110*	0.227***	0.152**	0.134***	0.158***	0.220***
	(0.063)	(0.055)	(0.059)	(0.031)	(0.050)	(0.065)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	319	277	163	319	277	163
R^2	0.18	0.22	0.15	0.14	0.17	0.25

Note: *WG high*: binary indicator of high exposure to wartime governance (1 = Yes, 0 = No). *Std. controls*: same specification as in column 1 of Table 5. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, in columns 1 and 3 clustered by municipality of recruitment, in columns 2 and 4 two-way clustered by *bairro* of residence and municipality of recruitment.

Appendix B

Appendix Chapter 4

B.1 Data and variables

We describe here the data, including the key variables and their construction used in the paper.

B.1.1 Data collection (POEMA)

Quantitative fieldwork started in March 2013 and ended in February 2014. It included survey data collection in partnership with the local Angolan NGO Development Workshop (DW). While NGO activity is generally very low in Angola, DW has operated in Angola and Huambo Province for more than 25 years and was instrumental in making this survey possible. Importantly, DW have not implemented aid or other development programs related to domestic violence or demobilized soldiers. With DW's support, the authors recruited, screened, trained, and managed a team of enumerators to conduct interviews based on close-ended questionnaires coded on tablet devices.

To ensure as representative a sample as possible, the survey employed three levels of randomization, where the first two involved the primary sampling unit (PSU) and enumeration area (EA) levels. A PSU (*bairro*) is in many rural areas the *comuna* (the lowest tier formal administrative unit), and in urban areas often equivalent to a borough. An EA (*village*) is in rural areas either one settlement or multiple very small settlements, and in urban areas equivalent to a neighborhood. In the absence of systematic and reliable veteran population data, we used up-to date data of the total population from the ongoing Angolan census to randomly draw PSUs and EAs, with weights proportional to the population. The sample was stratified by rural and urban locations, according to aggregate census estimates. At each survey site, we engaged with village chiefs, traditional authorities and local

administrations to produce listings of all former soldiers residing in the enumeration area (EA). Results were cross-checked to develop credibly complete listings of the local veteran population. Conditional on the reliability of the general population as a proxy for the ex-combatant population, as used in the first stages, the sampling strategy is self-weighting and ensures that the geographic spread across the province is representative. Assuming that we did obtain complete lists of ex-soldiers, the EA-level sample is representative of the EA-level veteran population.

We received IRB approval for this research, including all survey modules, under IRB Services Protocol USIP-070-10F (6659). The survey design followed standard protocols, was advised by a team of psychologists and – wherever possible – used existing modules that were adapted to the local context. We describe the modules underlying our key outcome and treatment variables below.

B.1.2 Data

Domestic violence indicator (outcome). Our main outcome variable is a domestic violence indicator built from questions on eight specific acts of domestic violence, using the questionnaire and following the protocols of the recommended Demographic and Health Surveys Domestic Violence Module (DHS, 2016). The eight specific acts include sexual and non-sexual physical violence and are listed in Appendix Table B.3. As suggested by the literature, our main outcome variable is a binary measure (e.g. La Mattina, 2016). The binary indicator equals one if the veteran's partner reports that she experienced any form of abuse by the veteran in the last 12 months, based on eight specific forms of violence. In addition, we will also analyze a binary indicator of severe injuries suffered from domestic violence over the past year.

Exposure to wartime sexual violence indicator (treatment). Our key explanatory variable is an indicator of wartime exposure to sexual violence against women, based on Maia et al. (2011) and Brück et al. (2016). In most specifications we use a binary measure, which equals one if the veteran reports having exposed to a situation where a civilian woman was abused (during the war). We also collected data on how often the veteran reports having been exposed. While we prefer the binary indicator as it is more reliable in terms of measurement error, we also present results on frequency of exposure. For ethical and legal reasons, the questions did not probe the degree of active or voluntary participation.

Army and year of birth indicators. *UNITA* is a dummy indicator for a veteran's main armed group. It equals one if his main army was UNITA; zero means MPLA. We create pooled year of birth indicators, each bin spanning five years, and truncated at 1950 and 1980. This results in eight pooled birth dummy variables, Bin1

means born before 1951, Bin2 between 1951 and 1960, up to Bin 8, meaning born after 1980. The cut-off at 1980 is motivated by the fact that someone born after 1980 is very unlikely to have joined an army before 1994, and on the other hand extremely likely to have served in the final period of the war from 1998 to 2002.[1]

B.1.3 Summary statistics

Table B.3 reports summary statistics for our main variables of interest and principal control variables. 44% of respondents' partners report having experienced (any form of) domestic violence in the last 12 months. Among veterans who report having been exposed at least once, 85% report total numbers of situations of five or less, 2% state numbers of twenty or more (not shown). 21% report having been exposed to at least one situation where a civilian woman was abused during the war. The average respondent is in his late forties and served in the military for about 11 years during the war.[2]

B.1.4 Clustering

An interesting question from a development point of view is whether exposed veterans cluster in specific villages. In extreme cases, we may have villages with shares of zero or one of exposed veterans. A priori we do not expect 'extreme villages', as about three-quarters report that they returned 'home' (see Table B.3) and – due to the exposure mechanism – the distribution of exposure among veterans from the same region should be similar to that the full sample. Figure B.1a confirms that we do not observe extreme villages, and the village-level shares of high-exposure types are relatively narrowly distributed around the overall individual mean of being an exposed veterans (of about one quarter).

Similarly, Figure B.1b plots the village-level share of perpetrators of domestic violence (of all sampled veterans in the village). As above, we find that the distribution is centered around the individual-level mean of around one half. The tails of the distribution again reveal that there is no village where either all sampled partners in a village report domestic violence (share=1) or none of them (share=0).

[1] All results are not sensitive to these cut-offs.

[2] The median age is 49 years; the median tenure is 9.58 years.

B.2 Additional results

B.2.1 OLS

To account for potential intra-cluster correlation in the error term we report in our main specifications Cameron, Gelbach and Miller (2011) standard errors two-way clustered at the locations of recruitment (municipality; N=38) and current residence (PSU; N=22). Appendix Table B.4 presents p-values of our coefficient of interest based on alternative standard errors. In column 3, standard errors are one-way clustered by the location of current residence, column 4 estimates standard errors are one-way clustered by the location of pre-service residence. Column 5 presents Huber-White robust standard errors. Because the numbers of clusters may be 'small' (see Cameron, Gelbach and Miller, 2011, e.g.) a potential concern is that we incorrectly inflate (or deflate) standard errors as a consequence of clustering. Column 6 presents p-values based on one-way clustered standard errors parametrically corrected by the Moulton-method (see Angrist and Pischke, 2008, e.g.), column 7 reports standard errors estimated by the wild cluster bootstrap method.[3] The estimated standard errors do not vary noticeably across these methods, and all result in comparable confidence intervals with p-values (well) below .01.

As our main outcome variables are dichotomous, we test whether our main results are robust to non-linear model specifications. Table B.5 reports average marginal effects estimated in a logit model. Across specifications, these are very similar – in significance and magnitude – to the effects found in the linear probability model.

B.2.2 Mechanisms

Relative economic bargaining power. Table B.9 reports direct effects of exposure to sexual violence on specific outcomes of or related to economic bargaining power. The results provide further evidence against a decisive role of economic bargaining power. We find no significant effects on the veteran's cash income (in logs), his labor force participation, and various measures of relative cash income (relative to his partner's).

Marriage formation and characteristics. Next, we investigate the impact of exposure to sexual violence on marriage market outcomes. Differences in marriage market outcomes could reflect mediation mechanisms based on gender norms and/or

[3] We use Rademacher weights (+1 with probability 0.5 and 1 with probability 0.5) for re-sampling residuals and impose that the null hypothesis of zero treatment effect as recommended by Cameron, Gelbach and Miller (2008). While frequently used, it should also be noted that this method is only reliable for large sample sizes.

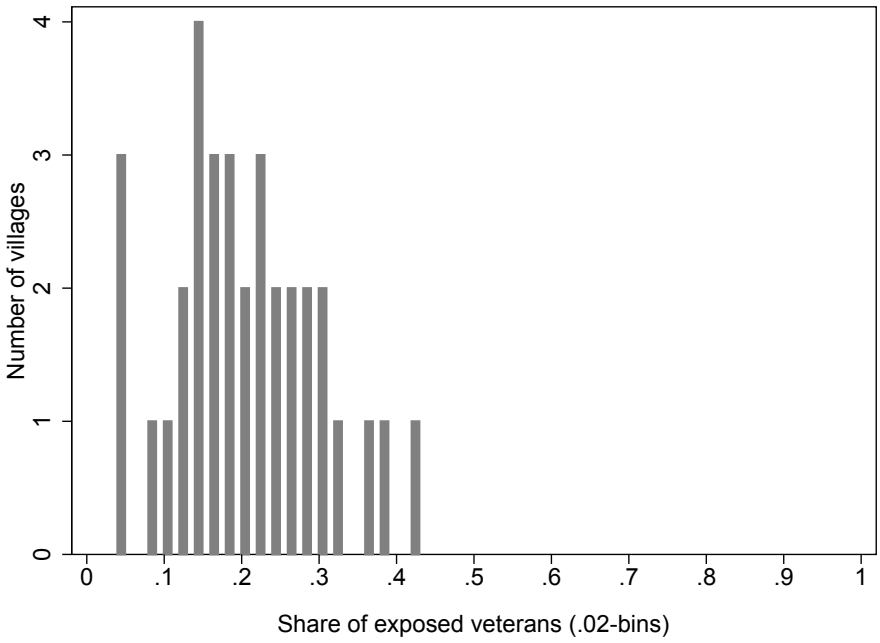
economic bargaining power. In either case, we expect that exposed would 'choose' or be married to relatively 'weak' partners, i.e. partners they will be able to dominate more easily, in terms of controlling their behavior or relative bargaining power.[4] Yet, the results displayed in Table B.10 find no such systematic differences in outcomes between exposed and non-exposed veterans. In terms of marriage outcomes, exposed veterans are not significantly more likely to be married, have more wives or wives that disagree with the statement "A woman has the right to refuse sexual intercourse with her husband if she does not want to have it" (columns 1 to 3). Rather, wives of exposed veterans are slightly *more* likely to agree with such a statement (column 3), yet are significantly less likely to have known their spouse before marriage (column 4). We see only very small and insignificant differences in terms of other dimensions of marriage formation, including land the woman brought into marriage, her occupation and her age at the time of marriage (column 5 to 7). Exposed veterans are about 4 percentage points more likely to have paid a bride price, which 62% of all veterans did according to their spouse (column 8). Ultimately, column 9 tests whether exposed veterans are more likely to marry a previous victim of sexual abuse (by any other person), which would imply that exposed veterans may pick "victims." Yet the results suggest that this is not the case. The coefficient is small and slightly negative. In sum, we observe that the effects on marriage market outcomes are weak and unlikely to be systematically affected by the exposure to wartime sexual violence, via gender and bargaining power channels.

B.3 Additional figures and tables

[4] As many other armed conflicts, the Angolan war took the lives of many more men than women. In addition, this logic builds on couples that formed after the end of the soldier's military service. All results are presented for the whole sample, but remain quantitatively unchanged for the sub-sample of unions established after the man's military service (not shown).

Figure B.1: Histograms at the village level

(a) Exposure to wartime sexual violence (share within village)



(b) Domestic violence (share within village)

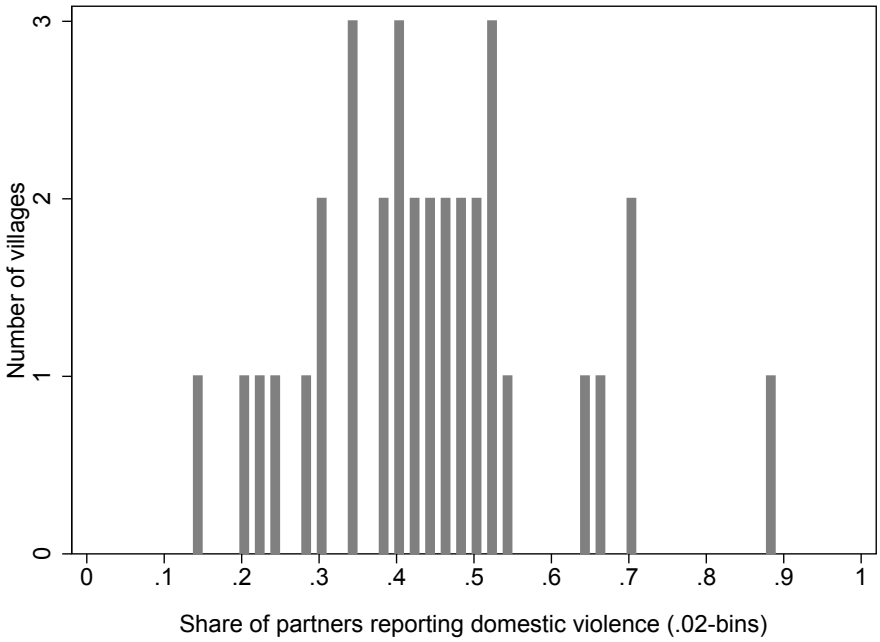


Table B.1: Selection into MPLA (vs UNITA)

	MPLA first army	
	(1)	(2)
Household size	0.002	0.003
	(0.823)	(0.645)
Church = Catholic (d)	−0.006	0.039
	(0.934)	(0.605)
Church = IECA (d)	0.045	0.025
	(0.572)	(0.752)
Number of rooms	−0.010	0.009
	(0.542)	(0.585)
Land size	0.005	0.002
	(0.578)	(0.798)
Livestock	0.002	0.002
	(0.376)	(0.352)
Farmer(d)	−0.102	−0.037
	(0.212)	(0.655)
Radio (d)	0.047	0.023
	(0.244)	(0.550)
HH members had joined (#)	−0.001	0.005
	(0.969)	(0.722)
Ever displaced (d)	0.023	0.027
	(0.565)	(0.485)
Father's schooling (d)	0.016	0.042
	(0.713)	(0.334)
Mother's schooling (d)	−0.001	0.004
	(0.988)	(0.922)
Pre-Location FE	No	Yes
Observations	578	578

Note: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Classical standard errors.

Table B.2: Missing data on domestic violence

	Intimate partner data missing					
	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	Full sample	Sub-sample: Has partner	Sub-sample: Has partner	Sub-sample: Has co-habitant partner	Sub-sample: Has co-habitant partner
War sex. viol.	−0.033		−0.031		−0.030	
	(0.028)		(0.032)		(0.033)	
MPLA x YOB-B. 3/7/8		−0.009		−0.015		0.008
		(0.069)		(0.068)		(0.070)
Age+age^2	Yes	Yes	Yes	Yes	Yes	Yes
Pre-controls	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	725	725	716	716
R^2	0.09	0.09	0.10	0.10	0.10	0.10

Note: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table B.3: Summary statistics

	Mean	SD	MIN	MAX
Domestic violence in last 12 months	0.44	0.497	0	1
Eight individual acts				
Pushed you, shook you, or threw something at you	0.35	0.479	0	1
Slapped you	0.43	0.495	0	1
Twisted your arm or pulled your hair	0.15	0.360	0	1
Punched you with his fist or an object	0.24	0.428	0	1
Kicked or dragged you	0.28	0.447	0	1
Tried to choke, strangle or burn you	0.12	0.320	0	1
Threatened or attacked you with a knife or other weapon	0.06	0.233	0	1
Physically forced you to have sexual intercourse	0.12	0.327	0	1
Key explanatory variable				
Situations where a civilian woman was sexually abused	0.21	0.410	0	1
Other key variables				
Age	49.56	9.682	26	86
MPLA (main)	0.70	0.459	0	1
Length of military service (yrs)	11.17	6.763	1	32
Born here	0.73	0.442	0	1
Years of schooling	3.89	2.773	0	12
Radio	0.73	0.444	0	1
Asset index	0.01	1.063	−1	10
Cash income (log)	6.27	3.877	0	13
Woman's cash income (log)	7.48	2.807	0	11
Woman earns more	0.49	0.500	0	1
Observations	578			

Table B.4: Alternative standard errors

	(1) CGM Village Pre-mun 2-way	(2) CGM Comuna Pre-mun 2-way	(3) CGM Village 1-way	(4) CGM Pre-mun 1-way	(5) Huber -White Robust	(6) Moulton Village	(7) Wild cluster bootstrap Boot-cluster: village 2-way
War sex. viol. [p]	0.0059	0.0075	0.0043	0.0089	0.0075	0.0127	0.0038
Post-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578

Note: Covariate specification from Table 1, column 3.

Table B.5: Non-linear model specification

	OLS			Logit (AME)			Logit (ME at means)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
War sex. viol.	0.119**	0.121**	0.109**	0.118**	0.122***	0.110***	0.129**	0.141**	0.127**
	(0.050)	(0.048)	(0.052)	(0.049)	(0.035)	(0.038)	(0.055)	(0.055)	(0.060)
Civilian targetting	No	No	Yes	No	No	Yes	No	No	Yes
Post-Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578	578	578

Note: Results from OLS and logit estimation (average marginal effects and marginal effects at means). Village-level fixed effects, Huber-White standard errors in parentheses (logit results to be analyzed with caution).

Table B.6: IV check 2: Correlation with pre-service variables

	MPLA x Bin3/7/8		MPLA first army			
	(1)	(2)	(3)	(4)	(5)	(6)
			Bin3/7/8	Bin3/7/8	Bin3/7/8	Bin3/7/8
Household size	−0.002	−0.003	−0.000	−0.002	−0.004	0.002
	(0.636)	(0.476)	(0.988)	(0.772)	(0.784)	(0.739)
Church = IECA (d)	0.007	−0.001	0.131	0.023	0.024	−0.014
	(0.827)	(0.986)	(0.138)	(0.626)	(0.782)	(0.747)
Number of rooms	−0.004	−0.001	0.015	−0.015	0.014	0.006
	(0.743)	(0.912)	(0.693)	(0.419)	(0.716)	(0.717)
Land size	0.006	0.006	−0.010	0.010	−0.002	0.007
	(0.404)	(0.367)	(0.633)	(0.316)	(0.929)	(0.460)
Livestock	−0.000	−0.001	0.004	0.002	0.001	0.002
	(0.817)	(0.608)	(0.493)	(0.514)	(0.897)	(0.353)
Farmer(d)	−0.032	−0.039	−0.254	−0.059	−0.132	−0.014
	(0.569)	(0.497)	(0.174)	(0.510)	(0.479)	(0.871)
Radio (d)	−0.021	−0.030	0.085	0.022	0.024	0.005
	(0.450)	(0.279)	(0.323)	(0.627)	(0.784)	(0.913)
HH members had joined (#)	−0.001	−0.002	−0.009	0.003	−0.003	0.006
	(0.945)	(0.873)	(0.774)	(0.854)	(0.935)	(0.702)
Ever displaced (d)	−0.009	0.012	−0.027	0.048	0.016	0.049
	(0.741)	(0.668)	(0.752)	(0.292)	(0.854)	(0.232)
Father's schooling (d)	0.037	0.042	0.023	0.014	0.092	0.023
	(0.240)	(0.179)	(0.803)	(0.780)	(0.308)	(0.623)
Mother's schooling (d)	−0.046	−0.040	−0.002	−0.009	0.000	−0.005
	(0.147)	(0.216)	(0.987)	(0.857)	(0.996)	(0.913)
Pre-Location FE	No	Yes	No	No	Yes	Yes
Observations	578	578	157	421	157	421
R^2	0.01	0.06	0.05	0.01	0.19	0.22

Note: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Classical standard errors.

Table B.7: IV check 3: Correlation with other war experiences

	Fatal violence			Operations against civilians	
	(1) Fellow soldiers	(2) Enemy soldiers	(3) Civilians	(4) Looting	(5) Massacres
MPLA x YOB-Bin 3/7/8	0.017 [0.862]	0.059 [0.556]	0.016 [0.870]	−0.037 [0.708]	0.074 [0.449]
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes
Pre-controls	Yes	Yes	Yes	Yes	Yes
Pre-region	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578
R^2	0.11	0.06	0.06	0.07	0.12

Note: *Wartime sexual violence* equals one if respondent reports having experienced at least once a situation where a civilian woman was sexually abused, during wartime. *Fellow soldiers died* equals one if respondent reports having experienced at least once a situation where many or most fellow soldiers lost their life, during wartime. *Enemy soldiers died* equals one if respondent reports having experienced at least once a situation where many or most enemy soldiers lost their life, during wartime. *Civilians died* equals one if respondent reports having experienced at least once a situation where many or most civilians lost their life, during wartime. *Looting* equals one if respondent reports having experienced at least once a situation where his group strategically attacked, but not civilians, during wartime (e.g. looting). *Civilian massacres* equals one if respondent reports having experienced at least once a situation where his group strategically killed civilians, during wartime. *YOB-Bin x* denotes pooled year of birth-cohorts (reference bin:"1965 $>$ YOB $\leq$ 1970") *MPLA x Bin3/7/8* denotes the interaction of being MPLA and being born pooled year of birth-cohort 3,7 or 8. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in brackets.

Table B.8: IV validity check 4: Correlation with post-war variables

	(1) Woman earns more	(2) Any formal education	(3) Asset index	(4) Radio
MPLA x YOB-Bin 3/7/8	−0.009 [0.912]	−0.016 [0.763]	−0.088 [0.577]	0.019 [0.858]
MPLA+YOB-Bin	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes
Observations	578	578	578	578

Note: Woman earns more equals one if woman earns more cash incom. *Any formal education* equals one if soldier received any formal education. *Asset index* is a household assets measure, based on 19 items. *Born here* equals one if soldier lives in the comuna where he was born. *Radio* equals one if the household possesses a radio device. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in brackets.

Table B.9: Economic bargaining power

	(1) Cash income (log)	(2) In labor force	(3) Woman earns more	(4) Cash income difference (lev.)	(5) Woman's share of cash income
War sexual violence	0.475 (0.443)	−0.022 (0.013)	−0.012 (0.076)	−1449.533 (2669.447)	0.017 (0.052)
Length of Service	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	6.27	0.93	0.49	2147.07	0.57
Observations	578	578	578	578	549
R^2	0.23	0.22	0.14	0.12	0.15

Note: Column 1: cash income earned by veteran in last 4 weeks (log). Column 2: Indicator equals one if veteran is in labor force, zero otherwise. Column 3: Indicator equals one if woman earned more cash income in last 4 weeks, zero otherwise. Column 4: Absolute difference between man's and woman's earned cash income in last 4 weeks. Column 5: Woman's share of total earned cash income in last 4 weeks (observations excluded where both have no cash income). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table B.10: Marriage formation and outcomes

	Outcomes			Formation					
	(1) Married	(2) Wives	(3) Att. sex. rel.	(4) Not arranged	(5) Land	(6) Occu-pation	(7) Age	(8) Bride-price	(9) Victim
War sex. viol.	−0.030	0.014	0.097*	−0.058***	−0.001	−0.006	−0.939	0.042*	−0.024
	(0.027)	(0.014)	(0.057)	(0.023)	(0.088)	(0.019)	(0.653)	(0.025)	(0.030)
Length of Serv.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.90	0.15	0.52	0.23	0.29	0.90	19.04	0.62	0.06
Observations	577	515	577	517	517	517	472	517	577
R^2	0.23	0.14	0.19	0.23	0.16	0.23	0.18	0.19	0.17

Note: All measures reported by veteran's partner. *Married* equals one if couple is married, traditionally or legally, as opposed to other unions as "união de-facto." *Wives* equals one if veteran has other partners. *Attitude sexual relations* equals one if veteran's partner agrees or strongly agrees with the statement: "A woman has the right to refuse sexual intercourse with her spouse if she does not want to have it." *Brideprice* equals one if veteran paid brideprice. *Land* equals one if spouse brought land into marriage. *Occupation* equals one if spouse was a household worker before getting married, including work for the household and domestic work. *Age* is spouses age when the couple got married. *Arranged* equals one if spouse did not know veteran before they got married. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

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Selbständigkeitserklärung

Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Berlin, 08. März 2016

Wolfgang Stojetz